ESSAYS
ON THE SOCIAL HISTORY
OF SCIENCE

# ESSAYS
# ON THE SOCIAL HISTORY
# OF SCIENCE

EDITED BY

S. LILLEY





CENTAURUS
VOLUME 3, 1953
NUMBER 1-2



EJNAR MUNKSGAARD
COPENHAGEN

PRINTED IN DENMARK
ANDELSBOGTRYKKERIET I ODENSE

Received from M/s Popular Book Depot Bombay on 24-2-55 for Rs 30.00

## CONTENTS

*The following papers deal with the social aspect of the history of sciences, a matter on which opinions differ very much. The editors have felt that it might interest the readers to have this aspect illustrated from many sides by a rather heterogenous collection of papers. Whatever opinion one may hold on this way of regarding the history of science, the trend to do so is a reality which should be studied.*

Mogens Pihl

## *INTRODUCTION*

*Before trying to define the purpose of this book, I must give some explanation of the administrative machinery to which it owes its origin. This will involve some high-sounding terminology of the kind relished by international organisations nowadays. There is an International Academy for the History of Science, whose administrative organ is the International Union for the History of Science. The joint function of these bodies is to sponsor and co-ordinate activities of international scope in the field of history of science. This work is carried out by commissions appointed by the Union. One of these is the Commission for the History of the Social Relations of Science, and the present book is an outcome of the activity of this Commission.*

*In matters of finance—which are, of course, of the first importance for the practical implementing of the Union's projects—most generous help has been forthcoming from UNESCO. In particular, UNESCO has shown an active interest in the work of the above-mentioned Commission, while allowing it to take its own decisions in complete autonomy. This has been a refreshing example of the kind of cordial and efficient collaboration which can be established between scientific and administrative organisms on an international level.*

*The undertaking of the preparation of the present book has actually been the Commission's response to a suggestion from UNESCO. It was felt that besides the usual kind of scientific work whose appeal is restricted to a small number of scholars engaged in special research, it was desirable to present before a wider public some of the problems which inspire these researches. The Commission gladly took up the hint, which was in complete harmony with its own preoccupations. As a matter of fact, the very decision to create a special Commission for the study of the social relations of science in their historical perspective demonstrates the importance attached by scientists and historians to this particular aspect of the history of science.*

*It is unnecessary to dwell on the topical interest of a study of the social implications of scientific research. The overwhelming part played by science in the development of modern society has become evident to everybody, not*

*Centaurus* 1953: 3: 7–11

*only by the way in which the applications of science affect the most varied aspects of everyday life, but also, unfortunately, by the ominous menace to the maintenance of civilized life which arises from the possible misuse of some results of scientific research.*

*In view of the urgency with which the relations between science and society thrust themselves into the centre of public interest, it might at first sight seem that a study of the social relations of science in the past is slightly academic. The opinion that history is the satisfaction of a somewhat idle curiosity is, however, quite mistaken. Its origin is rather recent: so recently was it a rather trite commonplace to speak of the "lessons of history" that it would be interesting to investigate in some detail the reasons for the neglect, and even disparagement, of history that is now so painfully and commonly noticeable in our society.*

*However this may be, the lessons of history are not simply those that can be drawn from more or less suggestive comparisons between similar situations which have arisen at different times. The analysis of their historical development is, in fact, quite essential for the understanding of all social phenomena and, in particular, of the social phenomenon of science. This comes from the essentially dynamic character of social evolution. No stage in this evolution can be isolated from the developments which preceded it, and which determined its characteristics. Any attempt at a static analysis of social relations without regard to the all-important time factor is doomed to failure. History does not simply provide an inspiring background to our thoughts on present problems: it supplies an essential part of the material which must be taken into consideration in order to make these thoughts really fruitful.*

*It would therefore be quite unfair to dismiss the enquiry into the historical aspects of social relations of science as just an academic appendage to the work of those organizations (such as the Committee for the Social Relations of Science, set up by the International Council of Scientific Unions) which are concerned with the present phase of these relations. The historical investigations which are the concern of our Commission aim at throwing light on the present situation by disclosing the complex interplay of events and ideas which link the present with the past and without whose recognition the present cannot be fully understood.*

*Perhaps some will think that I am stretching the case. To be sure, it would be difficult to argue that an analysis of Neolithic society and of the relation to that society of whatever rudimentary science existed then has more than a very slender bearing on the problems of modern society and modern science. But what this analysis gives us is insight into the continuity of social evolution*

*and the patterns according to which this evolution takes place; and neither the great law of continuity, which gives the whole evolution its trend and direction, nor the more detailed mechanisms of social evolution and the part played by science in them, could be grasped without going back as far as we can to the origins of this evolutionary process. Moreover, since the evolution of both society and science has been one of growing complexity, the elementary processes of evolution can be more easily disclosed and analysed in the earlier and simpler phases.*

*To the ambitious aim thus set to our studies, the present volume cannot claim to give more than a very modest contribution. It is only a first attempt to introduce a wider public to this kind of problem. It was felt that this task would have more chance of being successfully achieved by a series of essays giving selected examples of the way in which these problems may be tackled, than by a complete survey of the whole field. The planning of such a collection of essays and their publication was entrusted by the Commission to its secretary, Dr. S. Lilley, and I am glad of this opportunity of expressing the Commission's high appreciation of the manner in which he acquitted himself of this task. The long delay between the decision to undertake this publication and the production of the book is entirely due to accidental circumstances for which Dr. Lilley is not responsible, and which, in fact, increased the burden of his work in no small way. If the results of his efforts are not entirely in accord with his design, the blame is in no way to be laid on him, but rather on the intrinsic difficulties of a novel undertaking. It is hoped, however, that if this first venture, imperfect though it may be, proves to fulfil its aim, it will be followed by another attempt along the same lines which we would endeavour to make more comprehensive and better balanced.*

*In setting up the plan of the book, Dr. Lilley had taken special pains to distribute the essays, both in time and in subject, so as to cover as great a variety as possible of representative and critical periods in the historical development of science and to do justice to the ramifications of this development. It proved impossible, however, to find competent scholars to supply all the desired contributions, especially in the wide and little-explored field of oriental science. We were fortunate in persuading one of the most distinguished sinologues to write about the social relations of science and technology in China, but we failed to elicit similar assessments of the no less important aspects of the development of science and its social relations in the Arabic and Indian civilizations. Much against our wish, our selection, therefore, perpetuates the prevailing bias towards occidental culture which*

*to no negligible degree distorts the picture of the growth of human culture and obscures the striking unity of this growth.*

*Even the treatment of the evolution of science in European society in this volume is rather patchy; but still, the various contributions illustrate aspects of this evolution which are interesting not only in themselves, but also for the light they throw on the general processes governing the complex interaction between science and society. At any rate, we have been able to secure the collaboration of scholars of recognized authority on the various subjects. We owe them our special gratitude for their willingness to undertake the difficult task of condensing into the short space allotted to them the essential results of long and patient investigations. Some of the authors have chosen to give their references, others have preferred to omit them, but in every case we have a first-hand account based on carefully checked historical material. This confers upon these essays an intrinsic scientific value which goes far beyond that of the usual kind of popular exposition.*

*It goes without saying that the contributors have been left entirely free to choose their subjects and to decide upon the best ways of treating them. The Commission has no preconceived views to advocate. As a matter of fact, there is little probability that unanimity of agreement would be found among its members on many of the points made by the contributors to this volume. The Commission acts on the view—which is a vital requirement for fruitful scientific enquiry—that the truth can only impose itself by its own strength and that the greatest disservice which can be done to it is to try to force evidence into any rigid system.*

*This does not imply, of course, that there are no general principles to guide us in the analysis of the evidence about the social relations of science; but the point is that such general principles, here as in any other branch of science, can only emerge from the analysis of the evidence and cannot be drawn from any extraneous source. This may seem a trivial statement, but it will not appear superfluous to put especial emphasis on it when one thinks of so many painful examples of the way in which evidence in the field of social studies is distorted and misused to serve either emotional beliefs or deliberate political ends.*

*There is another, still more cogent reason which invites extreme caution in drawing general conclusions in the field of our studies. This is simply that we have hardly scratched the surface of a very complex subject and that we need a much greater accumulation of concrete data before we can be reasonably sure of discerning more than the broadest and most obvious features of the evolution whose mechanism we are trying to ascertain. Personally, I*

*belong to the more optimistic school of those who find that the evidence we already possess, scanty as it is, nevertheless suffices to bring out with great clarity the fundamental trend of social evolution and the position of science in it. To state it very briefly and inadequately, one is led to conclude that it is the development of the means of economic production which primarily determines the development of all other human activities, not excluding intellectual and spiritual development. I regret that so many prominent scholars are too timid to accept this general inference as a guidance in their studies, because I feel that they are thereby depriving themselves of a fruitful instrument of research. However, it is only by further careful and dispassionate analysis of more and more evidence that this issue can be decided. Thus it is on this task of the detailed analysis of concrete cases of social and scientific development that our efforts should be concentrated at the present time. At this stage the general outlook of the investigator matters comparatively little provided he controls it sufficiently to prevent it from obscuring his judgment.*

*The discerning reader of the essays in this volume will notice that in this question of method some of the contributors take a firm line, more or less inspired by the general principle just mentioned, while others are more hesitant and perhaps more inclined to lay stress on the finer shades which contribute to give social events an appearance of complexity. This variety in our approach to problems reflects the present state of our studies. One may think that it is a bad sign that a branch of science should be still uncertain about its method. On the other hand, it is certainly a good sign that the significance of the social relations of science, not so long ago utterly ignored by the majority of historians and scientists, is now fully recognized, and that their historical development is explored with an energy promising fruitful results.*

L. Rosenfeld
Chairman of the Commission
for the History of the Social Relations
of Science

# SCIENCE IN PRELITERATE SOCIETIES AND THE ANCIENT ORIENTAL CIVILISATIONS

by

V. Gordon Childe*

## I

"If by science be understood a body of rules and conceptions, based on experience and derived from it by logical inference, embodied in material achievements and carried on by some sort of social organization, then even the lowest savage communities have the beginning of science. Every primitive community is in possession of a considerable store of knowledge, based on experience and fashioned by reason"[1]. "Most of natural science was discovered in the neolithic age" if we mean thereby "the sort of natural science which is inseparable from an intelligent exploitation of the environment" and which involves "watching and remembering and handing down from father to son things which it is useful for a hunter or a shepherd or a farmer or a sailor or the like to know about the seasons, the weather, the soil, the stars ..."[2]. Let us accept these judgements of a distinguished ethnographer and a philosophical historian as justification for this essay and let us at the same time admit their definition of science: that it is, in effect, a traditional body of observations, expressed in mutually intelligible symbols (some sort of language) and providing rules for concerted action on a common environment—in other words a sort of ideal map or conceptual working model of reality serving as a guide to action.

We are not concerned with the content of "primitive science" but its structure is extremely relevant. For that sort of science implies, as much as modern Science, some measure of agreement both on the general nature of its object and on the principles on which the conceptual model

* D. Litt., D. Sc., F. B. A., Professor of Prehistoric Archaeology, University of London.

*Centaurus* 1953: 3: 12–23

shall be constructed. That is, it implies a common world-view and system of logic. Even today the "world view of science" would be an abstraction. Scientists profess widely differing philosophies—mechanism, materialism, spiritualism, idealism, holism, etc. But in practice all accept certain fundamental assumptions as to the nature of their object—notably the distinction between mind and matter. So few scientists now worry about logical theory, but all in practice use common categories—space, time, cause, number, class—and conform to approved ways of thinking which philosophers then formulate as "laws" of logic.

*Ex hypothesi* nothing can be known directly of the world view of prehistoric societies nor of the logical rules accepted by their members. Contemporary savage or barbarian tribes which resemble in their economy and equipment our own palaeolithic and neolithic ancestors not only exhibit enormous diversity in material culture and ritual, but differ from one another in their cosmologies and myths far more than modern scientists in their philosophies. Yet with proper precautions a comparative study of their behaviour yields as consistent a picture of the "primitive" world view and of "primitive" logic, as does a similar study of living scientists' behaviour. The application of these conclusions to prehistoric societies is justified by their agreement with the inferences of child-psychology and still more with the earliest recorded results of reflective thought, preserved in Mesopotamian and Egyptian literature.

In the "primitive world-view" as thus abstractly understood one feature is generally admitted. Our crucial distinctions between animate and inanimate, between men and brutes, between society and nature are never drawn[3] or at least neither sharply nor consistently. To that abstraction "the primitive" or "prehistoric man" "men, beasts, plants, stones, stars are all on one level of personality and animated existance"[4]. So, even in the IIIrd millennium B.C. an already literate Sumerian addresses common salt as a fellow human being: "O Salt, created in a clean place, For food of gods did Enlil destine thee ... O Salt, break my enchantment, loose my spell! Take from me the bewitchment! As my creator, I shall extol thee"[5].

All this does not mean that a thing or natural force was personified, had personality projected into it. It is apprehended as a person. The qualities that distinguish it are of the same kind as those which distinguish persons[6]. Its properties can be said to express its "will" provided we do not read into the term "will" a lot of modern metaphysics. A prehistoric flint-knapper, like his Sumerian counterpart, could say of his material "it will flake nicely", using "will" in its literal sense. In a word, as Frankfort[7] says

of the ancient Egyptians and Babylonians, man's relation to nature was an "I-Thou", not an "I-It" relation.

More germane to the present enquiry is the fact that nature was regarded as co-terminous with society, part of a single realm. That is implicit in totemism which, if not the most primitive and primordial form of all religion and philosophy, at least describes the systems of institutions observed among the technologically most primitive tribes and still reflected in the early historical myths of the Near East which depict the kosmos as a state. The totem—animal, plant, natural phenomenon, even artifact or act of man—is not only revered as the mythical ancestor of the clan. It is also treated as a member of the clan, a kinsman, just as much as human clansmen. Moreover other animals, plants or things that are not ancestors of any clans, are assigned to clans, just like men. Australian aborigines classify trees, smoke, stars and so on in totemic groups[8].

Thus nature in general is conceived on the model of society. If primitive man could construct for himself a conceptual model of reality, it would be built up on the analogy of his society; or rather it would be his conception of society itself. So of Mesopotamia in the IIIrd millennium B.C., Jacobsen[9] writes: "Intuitively the Mesopotamian applied to nature the experience he had of his own human society, interpreting it in social terms." Social structure would provide the analogy on which a model of reality could be constructed "in the mind". That is because in the "primitive's" environment society appeared the most predictable part and therefore that most amenable to change in a desired direction. At the same time the one thing every member of a "primitive" society has to know and which is constantly being dinned into him by precept and example, is his place in the family, the band or the village organisation. Social relations are thus inevitably common knowledge, and only common knowledge is a serviceable guide to concerted action.

Social relations again provide the principles of construction, the categories with the aid of which the concrete data of sensory experience can be built up into systems of knowledge or belief, into sciences. Durkheim[10] has offered a demonstration of the social origin of the categories of space, time, class and so on which need not be repeated here. For instance, "the notion of class is founded on that of the human group. But if men form groups, so also do things. A class is not an ideal, but a group of things between which internal relations exist similar to those of kindred ... A classification is a system whose parts are arranged according to a hierarchy. But the hierarchy is exclusively a social affair"[11].

The category of causality is particularly significant for science, and also for its social relations. To the primitive, if everything be alive, movement presents no problem since anything can move itself as easily as the thinking man. The practical question is, "how to make it move in the desired way?" Now an infant's early experiences show it that it can get certain parts of its environment to move—to bring it food or to remove sources of irritation—by appropriate gestures and noises which thus become symbolic acts[12]. So in adult life those fellow-tribesmen whom twentieth century anthropologists call human can be induced to act, although only within the limits rigidly prescribed by the customary behaviour pattern. Appropriate verbal appeals—spells—might on this analogy be expected to "cause" phenomena. On the other hand the exertion of one's own muscle power may just as obviously cause things and persons to move. Something of this physical idea still seems to some philosophers to lurk in the scientific conception of causality. But it is doubtful how far primitive men appreciate this implicit distinction between speech and action. "In its primary use language functions as a link in concerted human activity, as a piece of behaviour. It is a mode of action, not an instrument of reflexion"[13].

If the structure and form of embryonic sciences be so largely conditioned by the structure of society, it becomes relevant to enquire how prehistoric societies were organised. But that we frankly do not know. Archaeology alone entitles us to say only that the units were small. A neolithic village in Europe for example comprised from twenty-two to fifty-five huts of one or two rooms each, scattered about 2–5 acres and presumably housing 5 to 10 persons each. The preliterate societies described by ethnography exhibit a disconcerting variety of organisation. "Primitive society" as deduced by comparison is therefore an abstraction, just as great as the "primitive world view" or "primitive logic". Nevertheless all societies whose economy and technical equipment are comparable to those of palaeolithic and neolithic communities known to archaeology have one character in common: they can fairly be described as "simple".

That means they exhibit very little internal differentiation. Social division of labour is rudimentary or non-existent. There are normally no full-time specialists—no persons nor classes of persons who do not, as long as they are physically able, contribute actively and directly to the communal food supply. All are primarily engrossed in the urgent task of wringing a livelihood out of reluctant nature. Hence there are no classes with antagonistic interests. There are neither rulers nor

specialist crafts-men dependent for their livelihood on food produced by others.

But there may be differences of rank. Among the poorest hunters, like the Australians and Esquimaux, this depends on age. The old men are expected to take the lead, but everyone may hope automatically to attain this rank. Among higher hunters and many—but by no means all—cultivators, there may be chiefs, generally hereditary. But though such chiefs enjoy prestige and are expected to take the lead in all communal activities—war, hunting, gardening, building and so on—they are not thereby relieved of the physical work of fishing, hunting, or gardening any more than that of fighting; indeed without prowess and industry in food-getting their authority would vanish[14].

Similarly everyone is primarily engaged for most of his time in the acquisition of food, and everyone, or rather every household—for there is naturally a division of labour between the sexes both in food getting and in industries—is, and must be, able at a pinch to manufacture all such tools, weapons, receptacles (even pots), clothes, and ornaments as are indispensable. At the same time outstanding skill and expertness in craftsmanship, e. g., flint knapping, is recognised, socially utilised and rewarded even along the lowest hunters such as the Ona of Tierra del Fuego. "Though all men must spend most of their time hunting and so no one can set up as a professional artisan, even these Fuegians recognise special talent and honour it by the term 'master'. Such 'masters' have no regular customers, but they are paid for delicate work such as finishing off arrowheads"[15]. Among richer peoples this sort of specialisation is more common and extensive and may even lead to the rise of full-time specialists[16] dependent for their livelihood on exchanging their services or products for surplus food produced by others.

Such skills tend to be hereditary in families or clans in as much as the expert's children, natural or adoptive, have the greatest opportunity of learning them. Since the institutional family is normally larger than the biological unit, this method of transmission may result in the growth of craft-clans. But adoption is not uncommon though it involves rituals including a feast of the clansmen.

Ideally the science of a "primitive" community as defined at the beginning is public. In fact every child learns, albeit only through imitation and play, the essential simple techniques of making fire, hunting, gardening, building or manufacture, just as it learns to talk, walk, swim or wash itself. At puberty it is ceremonially initiated into this common traditional

lore of its group. On the other hand the craft skills of specialists are a source of prestige and profit to their owners, and, if only for that reason, tend to be made mysteries. Initiation can be secured only by adoption and only with the consent of all clansmen and in return for payment in the form of gifts and feasts. In practice this secrecy is guaranteed by the prevalent belief that the superiority of the craftsman is not due to acquired skill but to a mystic potency or *mana*, innate in its possessor[17]. Such belief is encouraged by the concrete, personal and imitative tradition by which craft lore, and indeed the whole of its science, is transmitted and perpetuated in a primitive society.

The education described in the last paragraph is not effected so much by the inculcation of verbal precepts as by practical example. The child imitates as closely as it can the actions of its elders, the novice tries to copy the motions and also perhaps the words and accessory gestures of the master. This makes tradition excessively concrete and personal. A formulated rule or a verbal description, however full, leaves out many details of the actual operation as irrelevant. The imitator is liable to reproduce slavishly every detail of the model. In fact, and even to-day, the practical lore and successful activity of hunters, farmers, sailors, miners, weavers, potters, smiths is often, perhaps normally, associated with and accompanied by irrelevant practices and futile actions or abstinences. Contemporary scientists dismiss them as queer superstitions, but in primitive societies they are regarded as essential and sanctified by magical beliefs.

All major activities of the group—wars, hunting, or fishing expeditions, the cycle of agricultural operations, the foundation of a settlement or house—for which its members know and apply perfectly efficient techniques are nevertheless preceded by elaborate ceremonies, the utterance of magical spells and by taboos, particularly abstinence from sexual intercourse, while spells and other symbolic actions accompany and reinforce the practical activities at every stage[18]. The same applies in varying degrees to the relatively more individual craft activities such as weaving, soap-boiling, pot-making, tree-felling, wood-carving and above all to the smelting and working of metal, and that whether the craft is plied by specialists or, like weaving and potting, is public[19]. So craft lore comprises not only familiarity with the proper materials and their effective manipulation, but also knowledge of the approved taboos, spells and rites.

At the same time the conviction of the efficacy and indeed necessity of these magical accessories reinforces and justifies the belief that the prowess

of warriors or huntsmen, the proficiency of weavers or smiths expresses and derives from a virtue or *mana* innate in their exponents and that this can be transmitted only by "blood"—in the mystical sense whereby ritual adoption constitutes a genuine bond of kinship. Moreover the very multiplication of magical expedients and precautions enhances the mystery of specialist craft-lore. The possessors of these secrets and bearers of such virtues are thus in a strong position over against the rest of society and can eventually exact a high price for their services and a still higher for adoption into the clan which is the sole means of initiation. It is no accident that "it is above all in those societies where skill in craftsmanship is highly developed, that importance is attached to magical precautions and ceremonies"[20].

Finally if magic thus pervade all practical activities, its special domain, where it proliferates most luxuriously, lies in those phases of human activities where knowledge fails man—in dealing with the weather, illness and so on. In prehistoric times and among primitive societies this domain was terrifyingly vast, but the urge to action nonetheless irresistible. However futile, socially approved ritual actions bring a necessary relief to emotional tensions. Yet claims, perfectly sincere claims of course, to be able to do what no one could in fact do then—or even now—are least amenable to the test of experience. So specialists in magic—and after all on the "primitive" view magical procedures do not differ in kind from craft operations—are in an even stronger position than professional craftsmen over against society. It may have been they who succeeded in transforming it[21].

## II

Literacy and with it exact, and therefore predictive, sciences arose in a quite novel form of society. For the first time in human history irrigation cultivation in the valleys of the Nile, and Tigris-Euphrates and the Indus permitted peasant farmers to produce a really substantial surplus of food above the requirements of their own domestic consumption while the rivers and canals made it easy to transport even bulky foodstuffs to urban centres. This surplus was used to support whole new classes of full-time professionals who did not grow their own food, but lived congregated together in cities, a unit of settlement of hitherto unprecedented magnitude[22]. But it did not reach them directly. The bulk of the surplus was delivered by the primary producers as tithes or taxes to a divine king (in Egypt) or

to a deity and his earthly steward (in Mesopotamia) and distributed and administered by his ministers[23].

Society was thus for the first time divided into economic classes with opposing interests, and this cleavage also involved a division between intellectual or mental labour and physical labour. On the one hand stood the lower classes who produced the food, dug the canals, built the temples and city walls, but were relieved of the responsibility of deciding when to start ploughing or how to lay out the canals. On the other hand the ruling class were relieved of physical labour in return for planning and organising the distribution of water, defence against aggression and above all the conciliation of divine favour. In this division most craftsmen, who thanks to the new social surplus could now be full-time specialists, were assigned together with primary producers to the lower classes. On the contrary, at least the more successful specialists in magic and leaders in war joined the ruling minority. But all alike were economically dependent on the temple or palace which controlled the granaries where the surplus food was gathered and the irrigation system which allowed of its production. In that sense the early Oriental states were totalitarian.

The concentration of wealth in the control of a divine king or a god and its administration by a corporation of officials evoked the need for an accurate and impersonal method of keeping records in conventional symbols that were durable and intelligible to all members of the corporation. Systems of writing and numeral notation had to be invented and were invented. The resultant cuneiform and hieroglyphic scripts were, however, very complex and cumbersome. Writing was a mystery, initiation into which required a long apprenticeship. The clerks or literati were therefore professionals, full-time specialists though belonging to the ruling class. The lower classes, including all the craftsmen who were the practical exponents of "primitive science", remained illiterate, and their craft lore was still transmitted and maintained in the old concrete, imitative way. The clerks, however, as a familiar Egyptian text puts it, were exempt from all manual tasks and heavy labour of any kind. They were thus cut off from the active contact with nature wherein the real contrast between nature and society is constantly being overcome by practice.

Yet to these clerks fell the task, not only of elaborating new mathematical, calendrical and astronomical sciences but also of formulating in abstract words the world view of their societies. The results have recently been analysed by Frankfort, Jacobsen and Wilson of the Oriental Institute of Chicago, and their account will be followed here.

The extant writings certainly do not express any comprehensive or consistent world view. The Babylonians' or the Egyptians' world-view was not formulated as a coherent philosophy, even if it had ever been thought out—which is most unlikely; like the "world-view of the primitive" it is an abstraction, inferred from very concrete myths, hymns and spells. As thus reconstructed it was very like the world view of prehistoric man as already indicated. Nature was still conceived as alive; man's relation to nature remained an I-Thou relation; the categories of space, time, causality and so on were not yet those of our logic; any conceptual working model of reality would be constructed, as before, on the analogy of society—the cosmos was indeed conceived as a state. But society had changed and so had the categories and the model.

Earthly society is divided into rulers who do no physical work, but issue orders, and servants who execute these orders in the sweat of their brows. So nature is ruled by gods, and these have servants who carry out their decrees and do the physical pushing and pulling. Order is imposed on nature by the will of the gods expressed in their ineluctable ordinances just as the order of society is maintained by and depends on the personal legislation of the monarch. The spoken command of the earthly ruler may really cause changes in the material environment so striking as the emergence of a pyramid or the conversion of a swamp into dry land. Naturally then the word of a god is an efficient cause. As rulers can be swayed by appeals and even bribes, so can the gods. Prayers and sacrifices would thus seem to be the most effective methods for securing desired ends. Hence the fantastic multiplication of sacrifices and the elaboration of liturgies.

The theory thus attributed to cloistered clerks, guaranteed by their societies leisure for contemplation and reflection, is, not surprisingly, more passive than that assigned to prehistoric savages and barbarians who were all actively engaged in changing the natural environment. Even their own ritual practice was not, it must be admitted, obviously consistent with that theory. Many of their most solemn ceremonies and rites like the sacred marriage seem to have been designed to secure directly "without the intervention of any spiritual or personal agency" (i. e., magically in Frazer's sense) the desired results—the fertility of fields and flocks, the rising of the sun[24]. Be that as it may, the oldest literate societies had maintained and elaborated magical techniques inherited from preliterate barbarism. These, rather than the effectual techniques of the crafts and the controllable processes of nature formed the objects of clerks' reflective thinking. Such

a cosmological myth as the Babylonian story of Creation reads more like the word-book of a magical drama or a commentary thereon than an attempt at a scientific explanation of the origin of the world based on observations of natural phenomena.

We are now in a position to discuss a question familiar in the history of science: "Did science originate in magic or in craft-lore?" The previous argument should have shown that the question is wrongly posed. The implied antithesis between craft-lore and magic, between technique and ritual, is an unhistorical abstraction. Historically the exercise of effective techniques is intimately mingled with magical practices, and technical skill is confused with innate virtue or *mana*. The concrete, imitative transmission of crafts[25], particularly in small homogeneous communities held together in a mechanical solidarity[26] by real or putative kinship, helped to perpetuate the confusion and impeded discrimination of effectual causes from incidental accessories. Writing, by making possible an impersonal and relatively abstract tradition should have been a corrective. But the hunters, shepherds, farmers, sailors and craftsmen who had "discovered science in the neolithic age" were relegated to the lower classes and remained illiterate; their "science" was not refined by abstract formulation. On the other hand the ruling classes who alone were literate and who monopolised intellectual labour, were withdrawn from productive action on nature. Indeed they owed their privileges and leisure to specialisation either in magical (ritual) or in destructive (military) techniques. It was therefore primarily magical lore (including the magical aspects of craft lore) that was first abstractly formulated in written words.

The accumulation in written form of magical astrological lore did no doubt at least provide data from which scientific astronomy could arise, but it took 1500 years[27] and perhaps inspiration from a differently organised society in Greece. At the same time the needs of civilised life had evoked within the ruling classes themselves new crafts—accountancy, surveying—and new practical techniques—arithmetic, geometry. These, as far as we can judge, were always quite free from magical or ritualistic admixture. Arithmetic and geometry in Egypt and still more in Mesopotamia were sciences without qualification. But they were also techniques invented and employed for practical purposes and at the same time quite novel techniques with no prehistory. On the contrary medical tradition, certainly originating in remote prehistoric times was not emancipated from its preliterate heritage of magic by committal to writing.

## NOTES

1. Malinowski, "Magic, Science and Religion", in Needham, *Science, Religion and Reality*, 1925, pp. 35 and 21.
2. Collingwood, R. G., *The New Leviathan*, par. 36, 32.
3. Some anthropologists would insist upon the reservation. So Lowie writes (*Cultural Anthropology*, 1934, 302) "Primitive men *are* capable of seeing the difference between living and dead matter ... But when emotionally wrought up they will invoke a pebble, a bit of charcoal or a lizard". It must be recalled that the concepts, "person" and "thing" are reciprocal "Into the idea they have formed of things primitive men have made human elements enter, but into the idea they have formed of themselves they have made enter elements from things". (Durkheim, *Elementary Forms of Religious Life*, 235).
4. A. Lang, "Mythology", *Encyclopedia Britannica*, 11th. ed., v. 19, p. 134.
5. Meier, *Die Assyrische Beschwörungstexte Maqlu*, VI, 111; English version by Mrs. Frankfort in *Before Philosophy*, 1949, 143.
6. G. Jacobsen in Frankfort, etc., *Before Philosophy*, l. c.
7. Frankfort, *op. cit.*, 13 ff.
8. Durkheim, *op. cit.*, 148.
9. *Before Philosophy*, 146–7.
10. R. K. Merton (in Gurvitch, *20th Century Sociology*, New York, 1945) complains that Durkheim has established the social origin, not of time and space, but of conventional divisions of them. But Durkheim had replied in advance "Space if purely and absolutely homogenous would be no use. It would not be what it is, if it were not, like time, divided up and differentiated" (*Elementary Forms*, 11).
11. *Ibid.*, p. 19, n. 2; pp. 142, 148.
12. Cf. Malinowski, *Coral Gardens and their Magic*, ii. 63.
13. Malinowski, in Ogden and Richards, *The Meaning of Meaning*, 474; cf. also, *Coral Gardens*, ii, 231.
14. So for instance, even among the relatively advanced Maori a chief was expected to be "industrious in collecting food, skilled in building houses, *pas* and canoes," etc. as well as "brave, learned in tribal boundaries, and wise in settling disputes" (Best in *J. Polynesian Soc.*, vii, 1898, 242). He normally possessed slaves, captured by his own prowess in war, but "he did not escape work by possession of slaves but in agriculture worked equally with his slaves". (Firth, *Primitive Economics of the N. Z. Maori*, 204) Even among the still more advanced Kayans of Borneo, the "chief gains little or nothing in the shape of material reward, but he may receive a little voluntary assistance in the cultivation of his fields", (Hose and McDougall, *Pagan Tribes of Borneo*, 1912, 65).
15. Lowie, *Cultural Anthropology*, 1934, 107.
16. So in Polynesia, tatooers, wood-carvers and canoe-builders tended to be "pure professionals" (Lowie, op. cit.,) Firth says that in New Zealand they devoted "the major part of their time to the one craft" (*Maori*, 207).
17. Thurnwald, *Economics in Primitive Communities*, 1932, 127.
18. Malinowski denies the pervasion of "primitives'" activities by magic, declaring that there is a clear cut division between the domains coped with by knowledge and work on the one hand, and by magic on the other. The domain of magic would be that

"of the unaccountable and adverse influence as well as the great unearned increment of fortunate coincidences" (in Needham, *op. cit.*, 31) "Magic appears in those phases of human activity where knowledge fails man—illness, weather" (*A Scientific Theory of Culture*, 1944, 198). He documents his assertion from the Trobriands where no magic is used in safe lagoon fishing, but describes in detail the continuous use of magic in deep sea fishing, gardening, boat-building, etc. His arguments, however, are by no means convincing. Cf. e. g., Lowie, *op. cit.*, 299.

19. This is admittedly not universally true, but is, I suspect, more generally true than the published material suggests. Those ethnographers who have described in detail primitive craft processes are generally more interested in the technical aspects than in technologically irrelevant accessories. Radcliffe Brown's observations (*The Andaman Islanders*, 1922, 179, 407, 443) that among the Andamanese the technical processes of knotting nets, making baskets and pots seem almost free of superstitious accessories, seem a reliable negative instance. For abstinences in connection with pot-making see Thurnwald, *op. cit.*, 130, 133—Bahira and Ewe in Africa—for the case of weaving in New Zealand, ib., 71. For comparable taboos connected with weaving in Malaya, I am indebted to Mr. Leach. The ceremonials, spells and superstitions connected with iron working are familiar even in Europe. The Indian evidence has been set forth recently with parallels from other areas by Elwin, *The Agaria*, 1942, esp. 130–169.
20. Thurnwald, *op. cit.* 35.
21. That the Egyptian pharaoh's prehistoric ancestors began as magician-chiefs like the "rain-makers" of contemporary Nilotic tribes has been argued very plausibly by Moret, *From Tribe to Empire*, 19.
22. The walled area of Ur of the Chaldees at the end of the IIIrd millennium was approximately 150 acres and its population has been reliably estimated as of the order of 24,000. Other equally reliable estimates range from 9000 (Eshnunna) to 19,000 (Lagash) (Frankfort, *Kingship and the Gods*, 1948, p. 96, n. 23)—so forty to a hundred times as big as a neolithic village!
23. On the character and consequences of the urban revolution see Childe, *Man makes Himself*.
24. So Jacobsen in Frankfort, *Before Philosophy*, 214–5.
25. Cf. Thurnwald, *op. cit.*, 134.
26. As defined by Durkheim, *de la Division du travail social*, 1902; as contrasted with organic solidarity that depends upon the differentiation of mutually complementary functions, it reposes on the similarity of all members of the group and is liable to disruption by differences of thought or practice.
27. I. e. till the 8th. century.

# METALS AND EARLY SCIENCE

by

R. J. Forbes*

If we want to assess the effects of pre-classical and classical metallurgy on the growth of early science, we must realise that the basic processes of metallurgy were discovered almost entirely during the pre-classical period, that is before 600 B.C. The study of early metallurgy reveals that it passed through different stages. These phases are summarised in the following table:

*Evolution of metallurgy.*

I. Native metal as stones.
II. Native metal stage (hammering, cutting, etc.)
(copper, gold, silver, meteoric iron).
III. Ore stage (from ore to metal, alloys,
composition as primary factor).
(lead, silver, copper, antimony, tin, bronze, brass).
IV. Iron stage (processing as primary factor).
(cast iron, wrought iron, steel).

It will be clear that the two earliest phases can hardly be called metallurgy. Only native metal was treated and hardly recognised to belong to a separate and peculiar class of "stones". Hence at first the usual wood- and stone-working techniques were applied, but finally some metallic properties played a part in this technique. These phases belong entirely to the prehistory of Europe and the Ancient Near East.

True metallurgy begins with the discovery of the "annealing" of native metals, which practically coincides with the important discovery of the melting and refining of copper and the smelting of oxide and carbonate

* Professor, Municipal University of Amsterdam, Netherlands.

*Centaurus* 1953: 3: 24–31

copper ores. The latter two complexes of discoveries ring in the true Ore Stage, which covers what archaeologists usually call the Copper Age and the Bronze Age, (their classification being based on the products of metallurgy rather than on the basic metallurgical processes themselves).

This true metallurgy, the recognition of the specific properties of metals and their ores, was born in the latter part of the fourth millennium B.C. in the Ancient Near East. The Ore Stage reached its full development when the smelting of sulphidic ores was discovered in the course of the third millennium B.C., probably in connection with the production of lead and silver from galena.

The Ore Stage covers the discovery of the production and refining of gold, silver, copper, lead, antimony and tin and their alloys. Technology profited most by the development of a series of bronzes with different tin content. Sometimes lead or antimony bronzes were used instead. This metallurgical phase was dominated by the production of alloys. Its specialisation by producing the appropriate alloy for each industrial purpose was intimately related with progress of refining technique, which allowed a more accurate dosage of the constituents than the earlier technique of mixing selected ores. The growing number of special alloys for different types of applications and the increasing quantitative accuracy with which the Bronze Age smiths progressively produced them are the indisputable proofs of this development. The composition of the metal or the alloy was the dominant factor in the production of the specific properties required in the tool or arm. Casting was the dominant technique, which also promoted the remelting and recasting of waste metal.

During this Ore Stage the working of meteoric iron and of iron ores was attempted. But the end-product remained useless until an entirely new complex of techniques and processes had been discovered. This was achieved about 1400 B.C. in the north-east corner of Asia Minor. The new metal, iron (that is wrought iron with a steel surface-layer produced by carburising) soon conquered the world as the diffusion of its production was helped by fortuitous political circumstances. Invaders from the Balkans shook and destroyed the Hittite Empire of Asia Minor, dispersing many of the iron smiths over the whole of the Near East. They also adopted the new technique themselves carrying it into Europe. Thus around 1000 B.C. the production of iron on a larger scale was well established in the ancient Near East and its adoption in prehistoric Europe began about the same time.

The study of early iron metallurgy reveals that the production of wrought iron and steel (here used throughout in the sense of surface-

carburised wrought iron) entailed the introduction of an entirely different complex of techniques and processes. The Bronze Age smith had to relearn his trade. The new techniques involved correct slagging of the matrix of iron ores, new tools and methods to handle the "bloom" produced by the first smelting of iron ores, and the mastery of the carburising, quenching and tempering processes, which enabled the new smith to produce steel from wrought iron. For only the new steel was superior to bronze and similar alloys—wrought iron alone would not have produced this technical revolution.

It is clear from the above summary that in the case of iron the final product was not so much determined by chemical composition (that is by the carbon content of the iron) as by the processes to which the wrought iron was subjected after its production. The iron smith and his tools and techniques are those that spring up in our mind when we mention the word "smith". We think of his hammer, bellows and anvil and no longer of the casting techniques of the Bronze Age smith. However, it should be realised that the full development of the Iron Age techniques was not reached before the beginning of our era.

The coming of the new metal, iron, made a lasting impression on the minds of the ancients. Not only did the meaning of metallurgical terms alter considerably—the Greek "chalkeus" originally a whitewright now came to designate a blacksmith. But far more important, the part played by metals in ordinary life was radically changed. Glover has aptly called the Bronze Age the age of princes and the Iron Age that of democracy. In the Ore Stage metals were not available in large quantities, mostly because the copper-hardening constituent, tin, could not be produced in sufficient quantities from the scarce and small deposits of tin-ores in the Near East. It had to be fetched from Cornwall, Bohemia or Spain and thus the use of bronze and its substitutes was restricted. On the other hand we must not judge from the archaeological finds alone, for much of the ancient copper and bronze was probably remelted and recast in Antiquity.

Iron was the first cheap metal produced by mankind, that was of general utility. It allowed the production of tools, weapons and armour for all instead of for princes and their retainers alone. The smaller workshops of the Bronze Age grew into important manufacturing centres in the Iron Age and stimulated the trade in ores and crude metals. Metallurgical skill was no longer restricted to the few but the number of smiths grew larger, mostly concentrated in the metallurgical centres. There arose strong guilds, which survived the crash of the Roman Empire.

Iron brought increased wealth to the craftsmen who made the tools and the arms for the many. Iron made living cheaper, for though corn prices are known to have risen in the earliest Iron Age, they fell rapidly soon after the political disturbances had quietened down. Only during the Hellenistic and Roman times did complications arise anew through the management of gold and silver currency, which brought back the "princes", and through the control over the armouries which became one of the pillars of government of the Roman Empire.

However, it is not our purpose to sketch the social effects of metallurgy. We want to trace some of its important effects on early science and technology. The effects are particularly clear in the latter case. The smith was one of the earliest artisans whose craft was a full time job. He could only emerge in a society, where agriculture produced a surplus of food to sustain such crafts, that is in the proto-historic period of the later fourth millennium B.C.

It is clear that the rapid development of metallurgy in the Ore Stage both induced and was effected by a rapid specialisation of the smith. This specialisation is summarised below:

*Evolution of the smith*

Primitive smith
from ore to finished product

miner
delving ores

metallurgist
processing ores

smelter
(ore to crude metal)

smith
(metal to mass product)

metal worker
(gold, silver, coppersmith)

Itinerant smith
gypsy, tinker

Copper and bronze metallurgy remained most important but the development of the production of lead, silver, gold, tin and antimony led to further specialisation after the early evolution of separate crafts of miners and

metallurgists. In each of these groups of smiths special refining and production processes, tools and techniques were evolved which had a strong influence on technology in general, however rudimentary real knowledge of the properties of metals might still be. We shall have occasion to point out the effects of metallurgical skill on the new art of alchemy.

The practical skill of these craftsmen and its influence on technology in general should not be underestimated. It also had great effects on the growth of the body of scientific knowledge. We must not forget that the very word metallurgy is derived from a Greek root metall-eia, "the delving for ores", which is closely related with metallao, "to search, to look for". We know that in the Ore Stage many itinerant smiths and prospectors travelled over the face of the Near East and Europe looking for surface deposits and veins of ore. Their trail can be followed by hoards or deposits of metal objects, cakes of crude metal, and cast-away material suitable for recasting. Their knowledge was of course restricted to the visible physical characteristics of metals and ores and to their behaviour in a few simple tests such as the "fire-test" and the reaction with acids like vinegar.

Yet at an early date such truly scientific data could be used by the Sumerians to classify natural objects on a sound basis. Though the terminology of metals and ores in other languages shows a similar selection of visible characteristics, it is especially pronounced in Sumerian nomenclature. The reason for this is the special agglutinative character of the Sumerian language. It enabled the ancient Sumerian prospectors and metallurgists to take a certain term for one class of minerals, say $^{a}$ZA for stones in general. To this root-word suffixes and prefixes were added describing the special characteristics of sub-groups and individuals belonging to the same class according to this primitive classification. Adding "GIN" (blue) to "$^{a}$ZA" would give "$^{a}$ZA-GIN", that is "blue stone"; and "$^{a}$ZA-GIN-AŠ" would be "hard blue stone". A statistical evaluation of the texts in which such words occur in ancient Sumerian or Akkadian shows us that these characterisations are extraordinarily correct and usually allow us to give the proper modern equivalent.

The system of classification thus achieved by the ancient Sumerians in the third millennium B.C. and later extended by the Akkadians shows much resemblance to that now used in organic chemistry. It proves that these early craftsmen made the most of such properties of metals and ores as they could observe with the simple tests at their disposal. This method of grouping of natural objects lived long. It was the basis of Theophrastus's

systematic survey of minerals, called *On the Stones*, of later medieval lapidaries and of modern systems.

But a study of these characteristics of metals and minerals had further effects. It led to the earliest quantitative analysis of metals and alloys which we call "assaying". Assaying was developed by the goldsmiths and the metallurgists of the gold and silver mines.

As early as 1500 B.C. we read about cupellation tests made on natural gold and electrum (the native alloy of gold and silver). This test was developed into a production process for pure gold and silver (salt process and other variants) and was also the earliest quantitative laboratory test. The "fining pot" is commonplace in the Bible, Egyptian and Akkadian texts and was well known in classical times. Another analytical test was developed in Lydia. The touchstone was a black stone on which streaks of gold of known and unknown composition could be compared. These two tests, and especially the latter rapid one, were fundamental to the creation of coinage, which in its true "mint" form issued from Lydia to conquer the world and develop trade and thrift.

The influence of metallurgy went much deeper than this alone. The facts arising from the experience of the ancient smiths and some correlations among them were absorbed into the body of ancient pre-classical science. This became apparent as soon as the study of the structure of matter and the reaction of chemical compounds found its shape in the last-born science of Hellenism, alchemy.

In these early chemical texts we find the results of the absorption of metallurgical experience into the world-picture of the ancients. It is clear from much earlier religious, magical and other texts that the craft of the smith excited great interest and above all awe from the earliest times onwards. It was known at an early date that meteoric iron came from Heaven. Names like AN-BAR and the Egyptian biȝ-n-pt call it literally the "metal from heaven". However, most metals and minerals were known to be products of the earth. The smith was the craftsman who produced these metals from "stones that grew in the Womb of the Earth".

The earliest smith's craft was a mixture of ritual and technique. The sacred art of the "metal-doctor" was placed on the same level as that of the witch-doctor. In fact examples abound in folklore all over the world in which magical powers are ascribed to the smith. The expert who could transform stones into metal could not fail to be a master of the powers of the earth. For the "mana", the power of the Earth, was transferred not only to the metal but also to the smith himself and even to his tools.

Purity and ascetism as well as the knowledge of the proper ritual were exacted from the smith performing his magical act.

The later alchemical belief in the sexuality of stones and metals goes back to this early metallurgy. For the new "charged stone", the metal, was born, and with birth the ancients coupled the idea of sexuality. Early Akkadian texts speak of "male" and "female" stones and metals. These different forms sometimes denote differences of texture or hardness.

More important was the early belief that stones and metal live in the womb of the Earth and there pass to perfection and even to death. The smith, who deprives these stones of their natural growth to perfection, somehow bypasses Nature's processes in his furnace and was thought to be able to obtain this perfect state by his magic. The natural evolution of the baser metals into silver and gold was part of his magic. This early transmutation lore passed into Hellenistic alchemy and was awakened in Arabic alchemy when kindred theories reached the Arabs from late Hellenistic and Far Eastern philosophy. The belief that stones and metals grew naturally in mines lived on until the nineteenth century and is probably still living in certain outposts of civilisation.

In early texts we find that the smith who tore these stones from the womb of the Earth, had to pay a penalty for this sin. He cancelled a paradisical state and changed "adamic" conditions. One life exacted another. Hence the Akkadian texts demand that an embryo be buried under the furnace to be built. This sacrifice of an abnormal birth fits into early belief. Was not the metal that the furnace produced itself an abnormal birth? The later alchemical tradition often considers the furnace as a vulva, as do many primitive smiths to this date.

The combination of these ideas and that of the sexuality of metals gives rise to the later theory of the male and female seed, represented by "mercury" and "sulphur" in Arab alchemy, which combine with the mineral to form the "child", the new metal. The ores or stones are considered to be the "genetrix" or "matrix" from which this child is born. Such ideas which can be traced in early metallurgical lore gave rise to the alchemical theories of the "marriage of metals" which is consummated in chemical combination. Other alchemical terms like "love" (combustion) and "death" (incineration) belong to the same class.

Still more features of early metallurgy appear in later alchemy. It is well known that the earliest alchemical reactions concern the colouring of metals. The reactions with these metals very often bear the same names as refining processes mentioned by the early metallurgists such

as "cooking" (bašlu), "washing" (misû) or "roasting" (kalû). Ancient nomenclature of metals abounds with distinctions concerning the colours of different forms of one metal with varying amounts of contaminations. At least half of the sixteen different Akkadian terms for gold are connected with some hue or colour of native and refined products. These associations of metals with colours were blended with further associations of colours with the gods and their stars or planets; and thus the later astrological and alchemical linking of god, planet and colour came into being. Again these colours of metals play a large part in the attempts of the early alchemists to imitate them by the "kerotakis" and other processes.

Thus while the practical recipes and techniques of the early smiths contributed to the development of technology in general, its magical and religious theory or background became part of the early pre-classical science. From that early science the strands lead us to alchemy, the youngest branch of science developed by the classical world. These beliefs stimulated the alchemists to enquire into the transmutations of metals and the reactions of chemical compounds collecting data which, in the eighteenth century, helped to build our modern chemistry.

## BIBLIOGRAPHY

R. J. Forbes: *Metallurgy in Antiquity* (Brill, Leiden, 1950).

— *Bibliographia Antiqua, Philosophia Naturalis*, part II (Institut voor het Nabije Oosten, Leiden, 1942).

# THE RISE OF ABSTRACT SCIENCE AMONG THE GREEKS

by

B. Farrington*

Since the attempt to write the history of science was begun in modern times strong curiosity has centred round the achievement of the Greeks in abstract science. They first attempted to determine the conditions requisite for the establishment of a general truth. They first tried to distinguish science from opinion. They first thought profoundly on the respective contributions to science of experience and reason. They first conceived the ideal of science as a body of knowledge logically deduced from a limited number of axioms. How did this remarkable development come about and what is its significance? These questions will be long discussed and much remains to be discovered about them. All I hope to do here is to suggest that with the development of what is now commonly called the sociology of knowledge we may confidently expect a better understanding of this vital phase of human thought.

In approaching our problem we shall begin with geometry which, it is generally agreed, was the typical scientific achievement of the Greeks. The rise of geometry as a purely theoretical discipline presents our problem in the sharpest and the clearest way. How, it is asked, did the transformation of the older practical geometry of the Orient into an *a priori* deductive science come about? By what miracle was the technique of land-measurement exalted into what Wordsworth aptly called

> an independent world
> Created out of pure intelligence?

Only a generation ago the best scholars were content to answer this question by a mere tautology. Sir Thomas Heath[1], for instance, answers in terms of "a special aptitude" of the Greeks. The Greeks had "a genius

* Professor of Classics, University College, Swansea.

*Centaurus* 1953: 3: 32–39

for philosophy." "Beyond any other people of antiquity they possessed the love of knowledge for its own sake." "A still more essential fact is that the Greeks were a race of *thinkers*."

If for these empty phrases we seek to substitute concrete historical realities our problem seems to split itself up into a number of different questions. For the very limited purpose of our paper we shall reduce these to three or four. First, while admitting that Greek mathematics is abstract in comparison with that of the older civilizations, we shall enquire whether the difference is not one of degree rather than of kind. Abstraction is characteristic of the whole development of the human intelligence out of the animal. In this process the most decisive stages were the invention of speech and of writing. Dismissing the former as too remote from our present enquiry we should note that the invention of writing, including a mathematical notation, belongs to Mesopotamia about the end of the fourth millennium. This new literate civilization marks such an advance in abstraction compared with the science of the neolithic village communities out of which it grew that it warns us that the title of this essay is possibly misleading. We must understand that the term abstract is used only in a relative sense and that, shifting our starting-point in time, we should be fully justified in writing an essay on the rise of abstract science among the Sumerians. Furthermore once the Mesopotamians and Egyptians had possessed themselves, for practical purposes, of a mathematical notation, this became, also with them, the starting-point for fresh theoretical advances.

"With the growth of ancient oriental civilization another development set in, which made mathematics divorce itself considerably more from the world of direct experience. This was the teaching of mathematics in the schools of the scribes, in the temples and administrative buildings of Memphis, Babylon, and other oriental centres. The schooling of the new administrators led to the formulation of abstract problems for the sake of training, and this already began to assume the form of a cultivation of mathematics for its own sake. It led to a new abstract approach to problems concerning number and space, in which algorisms, theorems, and theories emerged ... Mathematical relationships, which were found and studied independently of their direct applicability, were the result of the intrinsic logic and creative power of mathematics itself ... This development, thus started, came to full fruition in Greek mathematics"[2].

Abstract mathematics was, thus, not an absolutely original departure with the Greeks. It was the development of an already existing tendency.

Here also it is necessary to remember that, though growth in the power of abstraction is always synonymous with mental development, it has its own dangers. Accordingly it is refreshing to realise that the Greeks were by no means indifferent, at least not everywhere and always indifferent, to the empirical, applied mathematics of their oriental predecessors. We get a somewhat false picture when we formulate the problem as I have done on an earlier page. It is not a question of the transformation of a purely empirical oriental technique into a rational Greek science. It is a question rather of a shift of emphasis. What the Greeks came to value most was their achievement in pure mathematics, but we should beware of following them into contempt for their own brillant work in empirical and applied mathematics.

Modern historians have not always kept clear of this danger. Lancelot Hogben, for instance, falls into it in his famous book[3]. To explain what he calls the relation between size and social use Hogben gives diagrams showing how the same length of wall will enclose a greater or lesser area according as it is made to enclose a square or a parallelogram. "The Greek intellectual", he adds, "did not grasp this relativity of size and social usefulness." The Greek intellectual may sometimes have *despised* such knowledge; he certainly did not fail to *grasp* it. Pappus[4] credited even the bees with this capacity. He notes first how they adopt a shape for their cells which allows them to have their sides in common. Three regular figures, the triangle, the square and the hexagon, would satisfy this requirement. "But", adds Pappus, "bees know that the hexagon is greater than the square or the triangle and *will hold more honey for the expenditure of the same material in constructing the cell walls.*"

The quest for empirical formulae in applied mathematics is well illustrated by Philo of Byzantium[5]. He describes the application of mathematics to the construction of artillery. "Some of the ancients discovered that the diameter of the bore was the basic measurement in the construction of artillery. It was necessary to determine this diameter by a methodical investigation which would show the relation of this diameter to every other magnitude in the gun. This could only be done by varying the size of the bore and testing results. Such unlimited experiment was beyond the resources of the earlier engineers. Success has only recently been achieved by the Alexandrians, who were heavily subsidized by their kings, eager for fame and patrons of the arts. These Alexandrian engineers took note of the errors of their predecessors and the results of their own experiments and succeeded in reducing the

principle of construction, as has been said, to a single basic element, viz., the diameter of the circle that received the twisted skeins." There follows a table of these experimental results.

It is obvious to anyone who considers the actual material achievement of the Greek engineers that innumerable researches of the kind described by Philo must have been made. Such researches constitute the natural development of the applied mathematics of the Mesopotamian and Egyptian engineers. They are, moreover, typical of the early, as well as of the Alexandrian, period of Greek science. Many historians now stress the mathematical achievement of the early Ionians. The tribute of Archimedes to Democritus for his share in the solution of the problem of the relation of the cone to the cylinder and of the pyramid to the prism is revealing. "We should give him", he wrote, "no small share of the credit, for he was the first to state the relation although he could not prove it."[6] Nor can we refuse an empirical mathematical background to the great achievement of Eupalinus of Megara, the engineer who tunnelled the hill of Kastro in the middle of the sixth century.

Our problem, then, is as much to account for a development which took place within the history of Greek mathematics as to explain a contrast between Greeks and Orientals. We have to see the advance in abstraction in two distinct stages. First there is the Greek advance beyond the level reached by the older civilizations. Then there is the internal Greek development in the course of which the abstract side of science was exalted over the practical in a new and surprising way.

The earliest Greek scientific schools, those of the Ionian seaboard, like their predecessor in Egypt and Mesopotamia, developed their theory in close association with practice. It is probable, nay certain, that those Greek engineers whose achievements so excited the admiration of Herodotus (III, 60), made great advances on their oriental teachers. An immense social revolution had occurred since the priestly corporations of the old river-valley civilizations had invented writing and ciphering to aid them in the distribution of seed-corn and live-stock, in reading the stars, in keeping the calendar, building the temples, and training fresh armies of scribes. Iron-metallurgy had favoured the emergence of small independent city-states[7]. The phonetic alphabet had abolished the scribe's long training and democratised literacy. The consciousness of having invented not only new instruments of production but new forms of social life had fired the Greeks with the realisation that civilization is neither a gift nor a doom of the gods but a human achievement. On this realisation

followed the seemingly inexhaustible *élan* of the sixth century dawn of science. All knowledge seems to have a new edge as it shakes free of its mythological setting. An opinion is no longer regarded as right because it has always been held. It must be seen that it is so. On all sides reasons are given on the authority of the speaker himself. One aspect of this mental change is the new demand for proof as against rule-of-thumb in geometry. Those with an interest in mathematics are also the boldest speculators in other fields,—Thales, Anaximander, Democritus. But the evidence is that where the Ionian influence was strong theoretical and applied mathematics develop side by side.

It was among the western Greeks that the new emphasis on abstraction which connotes also a revolt from practice first showed itself. The change reveals itself in history as part of an educational reform; the educational reform reveals itself as part of a more comprehensive political and social program. These changes are associated with the Pythagorean movement. The words of the Greek historian Proclus are: "Pythagoras changed the study of geometry, giving it the form of a liberal discipline, seeking its first principles in ultimate ideas, and investigating its theorems abstractly and in a purely intellectual way"[8].

Perhaps it would not be too much to say that these are ominous as well as inspiring words. This conception of mathematics as a liberal discipline was obviously the prelude to an immense development in the field of pure mathematics. Equally obviously it connotes a social as well as a scientific development. Everybody now sees that the social development was not altogether wholesome. The opinion grows that the scientific development was not altogether wholesome either. Let us look at this complex development a little more closely.

If we are to understand the passionate emotion with which the Greeks launched themselves on the conquest of a science which should no longer be that of any profession—not that of the star-gazer, nor the pilot, nor the land-surveyor, nor the engineer—but simply that of the citizen; a science which should owe nothing to experience of things and all to reasoning about those mental concepts "which the soul by itself makes objects of contemplation, when it completely divorces itself from forms connected with matter"[9]; we have got first to understand the new conception of citizenship. When mathematics became a liberal discipline it became the hall-mark of the citizen in a slave society. Mathematics was what it was fitting for a man to know who was emancipated from the body-and-soul-destroying drudgery (to quote the Greeks' own description)

of the basic manual trades. What distinguished the citizen was the possession of reason. Geometry was systematised reason, pure reason, the citizen's science *par excellence*. The exaggerated importance attached to this aspect of geometry is explained when we remember that in this society the possession of reason was so literally accepted as the distinguishing character, not of a *man*, but of a *citizen*, that a slave was not allowed to give evidence except under torture. A slave must not be presumed capable of rational discourse; the words must be pinched out of him like mechanical effects.

The depth of the contempt for the slave is the measure of the exaltation of pure reason. This explains the insolence of Euclid who, when someone asked the use of a theorem, said: "Pay him for listening." This explains why Archimedes, whose mechanical inventions were famous, refused to leave behind him a treatise on mechanics for the express reason that the work of an engineer, just because it is useful, is vulgar. Such was the society which came in the end to decry and conceal its own practical achievement and succeeded in creating for itself an independent world out of pure intelligence. And since this world was the real world, the Creator of course was a mathematician. In the older oriental civilizations we find God beginning as a gardener who rescues cultivable land out of the primal slime, continuing as a potter, and ending as a ruler who makes things by giving orders, by his word. But it was in Greece, after Pythagoras, that God became a geometer. Though some modern philosophers still recognize him best under this denomination it is clear that the development of God from a gardener to a geometer, which parallels the development of geometry from land-surveying to a world of pure intelligence, epitomises the loss as well as the gain implied in the progress of abstraction.

For we have yet to mention the loss that Greek mathematics suffered by its predilection for the abstract, the deductive, and the pure over the concrete, the empirical and the applied. Where the positive achievement is so great criticism is apt to be silenced, and when it does presume to make itself heard it runs the risk of talking foolishly. There is, however, a judgment of Whitehead's on this point of first-class historical import, which is perhaps not so well known as it deserves. It puts better than any other comment known to me the paradoxical development of Greek mathematics owing to the violence of the Greek passion for abstraction. "It is a mistake to think that the Greeks discovered the elements of mathematics, and that we have added the advanced parts of the subject. The opposite is more nearly the case; they were interested in the higher

parts of the subject and never discovered the elements. The practical elements, as they are now employed in physical science, and the theoretical elements upon which the whole reposes, were alike unknown to them. Weierstrass' theory of limits and Georg Cantor's theory of sets of points are much more allied to Greek modes of thought than our modern arithmetic, our modern theory of positive and negative numbers, our modern graphical representation of the functional relation, or our modern idea of the algebraic variable. Elementary mathematics is one of the most characteristic creations of modern thought. It is characteristic of modern thought by virtue of the intimate way in which it correlates theory and practice"[10].

There can be no doubt about the brilliance and the importance of the Greek achievement, but it is most necessary also that we should appreciate its limitations. As Whitehead says in the same essay, "the effect we want to produce on our pupils is to generate a capacity to apply ideas to the concrete universe". The Greeks wanted to teach them to fly from it. Therefore, still following Whitehead, when we consider "the astounding success of modern science in transforming the world" we find ourselves compelled to admit that "in this region ancient thought is frankly useless."

It would seem obvious, indeed, that if the flight from practice occasioned a lop-sided development in the most abstract of the sciences, mathematics, it must have had a still more baneful effect on the more concrete sciences. The failure of the Greeks to create a science of mechanics is a case in point. Their practical achievement in this domain is not reflected in a corresponding body of theory. In the Aristotelian *corpus* there is a treatise on mechanics which is infinitely suggestive in the great variety of practical problems it raises and the tentative solutions advanced. Concepts of motion and force, conspicuously absent later, present themselves for clarification. The work seems the prelude to an applied mathematics of the wheel and lever in which theory would rest on empirical research. But, as we have seen, Archimedes turned from such studies and confined himself, so far as mechanics is concerned, to the theory of statics, where the logical element was greatest and the empirical least.

We may attempt in conclusion to sum up the few points we have tried to suggest in this brief discussion. (1) Abstract science was not an absolutely fresh departure with the Greeks but the development of a tendency already present in the science of the Near East. (2) The immense and fruitful development of the power of abstraction which characterises Greek science is to be explained rather by social than racial causes. Here we can only

hint at their nature. Roughly, iron-metallurgy and the phonetic alphabet made possible the independence of city-states in which appeared a new type of man, the first real citizen, an individual who felt himself responsible for the laws under which he lived, the rites he performed, the productive processes he controlled, and attempted to explain all these things to himself and his fellows. A statesman like Solon, a poet like Archilochus, a wise-man like Thales, are the products of this new society. It is to what these men said about themselves and about what they were doing that we refer when we talk of the Greek power of abstract thought. (3) When this new citizen class had come to regard mankind as split into two types, thinkers and workers, citizens and slaves, they gave a new meaning to abstraction. The power to abstract no longer meant penetrating into the laws revealed in practice, but flight from practice, flight from the work-a-day world, to an independent world of pure intelligence. Even this one-sided development had an enormous contribution to make to human progress by refining the instruments of thought—language, logic, mathematics. But these techniques lose their cutting-edge when they operate in the void. They must be applied to something and that again we might describe in Wordsworth's words as

> the very world, which is the world
> Of all of us,—the place where in the end
> We find our happiness, or not at all!

## NOTES

1. *Greek Mathematics*. Oxford, 1921. Vol. I, pp. 3–6.
2. Dirk J. Struik, "Mathematics" in *Philosophy for the Future*, Macmillan, 1949.
3. *Mathematics for the Million*, Allen and Unwin, 1936. Pp. 115, 116.
4. *Source Book in Greek Science*, McGraw-Hill, 1948. P. 81.
5. *Ibid.*, p. 318.
6. *Ibid.*, p. 70.
7. See V. Gordon Childe, *Story of Tools* (Cobbett, 1944), pp. 14, 15. The ability to smelt iron made metal cheaper, broke the monopoly of Bronze Age despots, and made possible the development of smaller and more progressive political units outside Egypt and Mesopotamia.
8. *Source Book in Greek Science*, p. 35.
9. This passage of Proclus, quoting the views of Geminus, might seem to be the origin of Wordsworth's lines. See *Source Book in Greek Science*, p. 2.
10. *Essays in Science and Philosophy*. Rider, 1948. Pp. 132, 3.

# THOUGHTS ON THE SOCIAL RELATIONS OF SCIENCE AND TECHNOLOGY IN CHINA

by

Joseph Needham*

One of the most fascinating questions in the comparative history of science, as it might be called, concerns the failure of the two great Asian civilisations, China and India, to develop spontaneously *modern* science and technology. It is unfortunate that their contributions to ancient and mediaeval science are not better appreciated, since only with that background in mind can the unique appearance of mathematised natural philosophy in Europe be comprehended. Before the fourteenth century A.D., Europe was almost wholly receiving from Asia rather than giving, especially in the field of technology. What can be said about the social milieu which produced that accomplishment and that failure?[1]

There seems no doubt that in early periods there was feudalism in China. It might perhaps be described as a "Bronze Age" proto-feudalism. It covered the period, roughly speaking, from the middle of the second millennium B.C. down to about 220 B.C., at which time the first unification of the Empire took place[2]. But from then onward, the use of the word "feudalism" seems more and more difficult because whereas the earliest period bears some resemblance to European mediaeval feudalism, the later periods are very different. The social system which emerged has been called Asiatic Bureaucratism, or, as some Chinese scholars prefer, Bureaucratic Feudalism. In other words, the ending of the first feudalism in China did not give rise to mercantile capitalism and industrial capitalism, but brought about instead a bureaucratic system involving the loss of the aristocratic and hereditary principle from Chinese society. What happened, one

* Sir William Dunn Reader in Biochemistry, and Member of the Faculty of Oriental Studies, University of Cambridge.

*Centaurus* 1953:3:40–48

might almost say, was that when the individual feudal lords of the intermediate levels ceased to exist, there remained only one great feudal lord, namely the emperor, governing and collecting taxes through a gigantic bureaucracy.

The members of this bureaucracy did not fully form a hereditary group, and so did not constitute a class in the customary sense of the word when used in relation to European societies. It was, as it were, an estate, and it had fluidity; families rose into it and sank out of it. As is well known, at a later period, entry was through the State examinations, a system which began during the Han dynasty, in the first or second century A.D., but did not attain its real flowering until the T'ang dynasty in the seventh century; and then it went on until the coming of the Republic in 1912. The examinations—again this is very generally known—were entirely based on literary and cultural subjects, and did not include subjects which could, in any sense, be called scientific,[3] but still, the examinations were quite difficult, indeed, when the extreme complexity of the Chinese language and literature is borne in mind, very difficult. But there were also, at different times and periods and in varying degrees, ways of getting around the examinations and entering the civil service hierarchy without passing through them. There was the "Yin privilege", by which the sons of bureaucrats were given an easier entry than those who came from outside, but on the whole, as far as individuals were concerned, the class was fluid. Families were rising into it and sinking out of it all through the centuries. It is also known that at some periods, the possibilities for a man of rural peasant family were quite considerable, and it was sometimes the custom for farmers to club together to pay for a tutor for some particularly promising young man in order that he might enter the Imperial service; investing, as it were, for benefits that would then accrue to his native place.

If one investigates the origins of this bureaucratic system which impressed its character so deeply on Chinese society, one comes upon several factors, geographical, hydrological and economic. The reason which has been given by the most eminent occidental economic historian of China, K. A. Wittfogel, for the origin of the bureaucracy was that it was conditioned by the immense and early growth of hydraulic engineering works in Chinese society. I found when I was in China, that that viewpoint is quite widely accepted by Chinese scholars who put, however, a somewhat different emphasis on it. The effect of the importance of irrigation and water-conservation works in Chinese history is indeed undoubted. Prob-

ably no country in the world has so many legends about heroic engineers, for example the legendary emperor Yü the Great, who "controlled the waters" for the first time in Chinese history. The rainfall in China is of course extremely seasonal, because it is a monsoon area, and also highly variable from year to year. When you consider how necessary irrigation was for wet rice cultivation in the centre and south and for the cultivation of the loess lands in the north, and when you add the constant flood danger requiring water-conservancy techniques, you see at once how extremely important these works were. We know that they started already in the feudal period (5th century B.C.). There is moreover a third reason why the water control system of the country was profoundly important, and that was because it provided a means of transportation. Since taxes were collected, or military supplies brought together in the form of kind and not money, the accumulation of rice and other grain at the capital required a method of heavy transport as by barges on canals. So there were three needs—irrigation, water-conservancy, and tax-grain transportation—which required a water economy to come into existence. Whereas western scholars have suggested that the origin of the "Mandarinate" could be traced to the fact that control had to be exercised over the millions of men who were brought together to carry out these works, many Chinese scholars whom I have read and listened to, consider that the deeper reason why this domination of the society by a "civil service" took place was because there was always the tendency to transfer control to central authority—in other words that the carrying out of water-work plans tended to transcend the boundaries of the estates of feudal lords. As a matter of fact, this is stated, in so many words, in one of the great Chinese books, the *Yen T'ieh Lun* (Discourses on Salt and Iron) written in 81 B.C.

This remarkable work, which reads like the records of a party conference, (I should say a Conservative Party conference) is, in fact, the dramatised account of one which actually took place about the nationalisation of the salt and iron industry, recommended as early as 400 B.C. and actually put into operation in B.C. 119. The Lord Chancellor opens one of the speeches by saying that we all realise that small local lords or governors are responsible for small amounts of territory, but the development of rivers, canals, and sluices must devolve upon the central authority. He was stating what was to remain a permanent feature of Chinese society. One of the earliest efforts of the Mandarinate was in fact the nationalisation of salt and iron in the former Han dynasty. These were the most important, perhaps the only, things that travelled from town to town. Everything else

could be made *in situ*, whether in weaving or the preparation of food, on the farm or in the local town, but salt and iron radiated from proto-industrial centres, salt from the sea coasts or brine fields, iron from the places where ore was found, and these were therefore the two commodities most suitable for control and "nationalisation". The interesting thing about the arguments which were put forward is that both the Confucian scholars who were criticising the Han bureaucrats, and the bureaucrats themselves, were violently against the merchants. There is, in fact, quite a mass of interesting evidence about the growth of a merchant community at the time when the first Ch'in Emperor unified the country and started the first centralised dynasty (230 B.C.). There is a special chapter in the *Shih Chi*[4] about the merchants of that time. Some were extremely wealthy; some were ironmasters, others were concerned with salt. Their power was immediately attacked by the early bureaucrats and rapidly destroyed. Sumptuary laws were enacted against them, and severe monetary taxes inflicted on them.

There is probably no other culture in the world where the conception of the civil service has become so deeply rooted. I myself had no idea of it when I first went to China, but you can find it everywhere there. Even in the folk-lore. Instead of stories about heroes and heroines becoming kings or princesses, as in Europe, in China it is always a matter of taking a high place in the examinations and rising in the bureaucracy, or marrying an important official. This was, of course, the only way in which to acquire wealth. There is a famous saying (current till recently) that in order to accumulate wealth you must enter the civil service and rise to high rank (*Tang kuan fa ts'ai*). The accumulation of wealth by the bureaucracy was the basis of the phenomenon often described by western people in China as "graft", "squeeze", and so on, and of which so many complained. The attitude of westerners, however, has been prejudiced by the fact that in Europe religion and moral uprightness had a historical connection with that quantitative book-keeping and capitalism which had no counterpart in China. At no time in Chinese history were the members of the Mandarinate paid a proper salary, as we should think natural in the west. There were constant efforts to do so, decrees were always being issued, but in point of fact, it was never done, and the reason is probably because the Chinese never had a full money economy.

Taxes had to be paid in kind, and transmitted in kind to the central authority, using the methods of fluviatile navigation to which I have already referred. It became inevitable that this tribute should be "taxed at

the source" (from the point of view of the emperor), and there are many expressions in Chinese for this state of affairs, one of the best being "*chung pao*" (middle satisfied), the point being that the peasants were not satisfied because of having to pay more than they thought they ought, and the emperor was not satisfied either, but the officials in the middle were quite satisfied because they were "taking a cut off the joint" at every stage. A special word with no moral connotations is needed for this phenomenon, to indicate that it was a natural feature of Chinese mediaeval society. When the bureaucrat,whether a magistrate of a city, or the governor of a province, or a *Chuang Yuan* with eight cities under his charge, had accumulated his capital, what he did with it, apart from expenditure on luxuries (and this would be quite natural in any large official family) was invariably to invest it in land. Land purchase was the only method of investment, and the result was a gradual increase in the number of tenant farmers. Before the overthrow of the Kuomintang, 40 or 50 per cent of the peasants were tenants, and most of their farms were uneconomically small.

I will now turn to another aspect of bureaucratic influence, which was always exerted against the merchants. The despising of the merchant was a very old characteristic in Chinese thought (and much in contrast with Arabic ideas); in the classical enumeration of the four ranks of society, the scholars came first, then the farmers, third the artisans and fourth the merchants; the merchants were supposed to be socially the lowest (*Shih*, *Nung*, *Kung*, *Shang*). There was of course in China nothing resembling the caste system, or even a class system in the orthodox sense of the word, but still, as a stratum of society, the merchants were certainly supposed to be the least socially respectable. It is nevertheless true that the merchants in China ultimately formed themselves into guilds, but one has to take a closer look at what they were like. I know something of them, because I have stayed in large houses belonging to merchant guilds. For instance, the University of Amoy set up its library during the war at Changting in a large house of many courtyards, which was the guild-house formerly used by the Chiangsi merchants who came to trade in Fukien. There is no question that there were guilds, but as several useful books have described them, they were different in many ways from the merchant guilds in Europe. They were more like mutual benefit societies, insurance organisations, protecting against loss occasioned in transit, and the like, but the one thing they never did was to acquire real control or power in the cities where the merchants lived and carried on their trades, or organised their small production workshops.

There was thus an essential difference between the guilds in China and those in the west, just as much indeed as there was between the city in China and the city in the west. Perhaps it can be summed up by saying that the conception of the City-State was unknown in Chinese culture and civilisation and the cultures that derived from it[5]. You have to set against the European conception of the City-State the Chinese conception of a City with its walls, surrounded by many villages from which the people come for the sake of market and trade, and with the headquarters of the magistrate or provincial governor appointed from the Imperial Court, responsible to no-one except his superior officials in the bureaucratic hierarchy. There would also be a military mandarin, and the two would have their offices in the town. It would, in a sense, be a walled, fortified city "held for the Crown" by the responsible local officials. There is nothing in Chinese history resembling the conception of a Mayor or Burgomaster, Aldermen, Councillors, Masters and Journeymen of guilds, or any of those civic individuals who played such a large part in the development of City institutions in the west. These things were quite unknown. A phrase comes to one's mind regarding the cities in the west "Stadtluft macht frei"—(A man can become free by entering the City and getting permission to live and work there). That is inconceivable in Chinese society. Another germane phrase would be "Bürgerliche Rechtssicherheit"—(Security under the Laws of the Boroughs)—the European merchants freely associating in their towns, and winning charters and advantages of all kinds from the feudal society which environed them. That is all foreign to Chinese culture and thought. Sir John Pratt has brought it out when he relates how the merchants in Shanghai about 1880 appealed to the Imperial Government in China for some kind of State Charter which would permit them to elect a Mayor or Burgomaster, Aldermen and so on, in fact to set up all the institutions associated with a City in the west. One can imagine the mystification produced at the Imperial Court at Peking when the request arrived there. Such lack of understanding was characteristic of both sides at the time.

There cannot be much doubt (as we can now see) that the failure of the rise of the merchant class to power in the state lies at the basis of the inhibition of the rise of modern science in Chinese society. What the exact connection was between early modern science and the merchants is of course a point not yet fully elucidated. Not all the sciences seem to have the same direct connection with mercantile activity. For instance, astronomy had been brought to quite a high level in China. It was an "orthodox"

science there because the regulation of the calendar was a matter of intense interest to the ruling authority. From ancient times the acceptance of the calendar promulgated by the Emperor had been a symbol of submission to him. On account of a great sensitivity to the "prognosticatory" aspect of natural phenomena, the Chinese had amassed long series of observations on things which had not been studied at all in the west, for example auroras. Records of sunspots had been kept by the Chinese, who must have observed them through thin slices of jade, or some similar translucent material, long before their very existence was suspected in the west. It was the same with eclipses, which were supposed to have a fortunate or antagonistic effect on dynastic events.

Then there were the "unorthodox" sciences, e. g., alchemy and chemistry which were always associated with Taoism. Neither astronomy nor chemistry could enter the modern phase, however, in the Chinese environment.

In the west the merchants seem to have been connected especially with physics, a science which in China had always been particularly backward except for the brilliant practical development of the magnetic compass. Perhaps this was due to the need of the merchants for exact measurements. The merchant could hardly carry on his trade without them. He had to take a lively interest in the actual properties of the things with which he was concerned. He had to know what sort of weight they were, what they were good for, what sort of lengths or sizes they came in, what containers would be necessary, and so on. Along such lines as that one might look for the connection of a mercantile civilisation with the exact sciences. But besides the merchandise there was also the transport. Everything which had to do with nautical construction and efficiency, was of interest, and had always been of interest, to the merchants of Europe's City-States[6].

If this is the case, it is precisely in the inhibition of the rise of merchants to power in the state that we have to look for the reasons for the inhibition of modern science and technology in Chinese culture. Another aspect of the matter is the old question of the antagonism between manual and mental work which has run through all ages and all civilisations. To Greek "*theoria*" and "*praxis*" correspond Chinese "*hsueh*" and "*shu*". It seems that no-one can fully overcome that tradition, no-one can advance to the point at which there is equal participation of hand and brain, so absolutely essential in scientific work, no-one can succeed in bringing them together, except the merchant class when it succeeds in imposing its mentality on

the surrounding society. That was simply never possible in China. There was a restriction of technology to an eotechnic level—seen for instance in the use of wood for gears instead of metal.

Yet here we have one of the most extraordinary paradoxes in history. Few people as yet realize what an enormous technological debt Europe owes to China during the first thirteen centuries of our era. While the old Chinese bureaucratic society was certainly inferior to the society of the European Renaissance in technical creativity, it had been much *more* successful than European feudalism, or the Hellenistic slave-owning society which had preceded it[7]. China contributed things like the efficient horse harness, the drawloom, the sternpost rudder, the first cybernetic machine, the earliest type of vaccination, and even so simple a device as the wheelbarrow—all these (when they travelled) came across from east to west, and not *vice-versa.* The strangest paradox is that the very people who by the nature of their society, if I am right in this diagnosis, were prevented from developing, as Europe did at the beginning of the Renaissance and the rise of capitalism, a state of society in which iron would become the basis of the first world-uniting civilisation, had, in fact, mastered the difficult art of iron-casting thirteen centuries before the West. We know that cast-iron was very uncommon there before the fourteenth century A.D. It may have been mastered occasionally by the Romans, but was certainly practised on a widespread scale by the Chinese in the first century B.C. It was in fact an ancient art in China, and the same is to be said also for the iron plough-share, and not only the plough-share, which travelled from east to west, but the mould-board as well. The Chinese were the first to introduce the mould-board—all this in a society unable to advance to the high metallurgical level of the later European societies.

If one asks who was the first to appreciate this difference between Asian and Western society, the answer might well be François Bernier, a French traveller who was physician to Aurungzeb, one of the last of the Mogul emperors. In his book he has some most remarkable pages. I was fortunate enough to get a copy of it in Calcutta and I shall always remember the excitement with which I read it. Written about 1670, in it is raised the question "whether it is an advantage or disadvantage to the State if the King is the owner of all the land, and not to have the *meum* and *tuum* which exists among ourselves". He came to the conclusion that it was a "disadvantage" for a country to have that type of society which we call Asian bureaucratism. He has a lot to say about the position of

what corresponded to the Mandarinate. In India, it was not exactly a Mandarinate, but still a civil service system, a non-hereditary bureaucracy to which appointments were made by the Mogul Emperors.

In concluding, one might suggest that Asian bureaucratism is by no means characteristic only of East Asia. There remains the great problem of Islamic science and society. As is well known, Arabic science was for 400 years much ahead of European. Now it would seem that the earlier Islamic society was really very mercantile. The Prophet himself has many words of praise for the merchants, but few for agriculturalists, and one might consider the Arab towns and cities on the edge of the deserts to be of a mercantile character, the desert taking the place of the sea. When the conquests took place, however, and the Caliphate was established in Bagdad, there came a movement to organize the mechanism of government more fully and introduce a much more bureaucratic state, similar to that which had existed in earlier times in Persia, and was nearer to the Chinese system. So perhaps what began in Islamic civilisation as a mercantile culture, ended by being thoroughly bureaucratic, and to this might possibly be ascribed the decline of Arabic society and particularly of the sciences and technology. But all that, of course, would be another story.

## NOTES

1. The present paper (which was submitted for publication in 1950) is concerned only with the Chinese situation. It forms part of a preliminary draft for one of the concluding portions of a book *Science and Civilisation in China*, the first four volumes of which are now in press with the Cambridge University Press.
2. The question of whether there was ever a stage of Chinese society depending on mass slavery is still controversial, but the concensus of opinion of western scholars is against it.
3. With occasional exceptions, as under Wang An-Shih in the Sung.
4. Ssuma Ch'ien's *Historical Memoirs*, written about B.C. 90.
5. The city-state conception may perhaps be applicable to certain small states in Central Asia, however (W. Eberhard).
6. This will need re-statement. Chinese nautical technology was much more advanced than that of Europe until the fourteenth or fifteenth century, A.D.
7. The elucidation of the social meaning of this fact constitutes a problem of importance equal to that which concerns the absence of a "Renaissance" in China.

# METALLURGY AND TECHNOLOGY IN THE MIDDLE AGES

by

R. J. Forbes*

The epithet "barbarian", attached to the Middle Ages by the Renaissance, has stuck tenaciously. Even though the historians of science and technology should know better by now, many of them still have at the back of their minds, the idea of the "Dark Ages" in which science and technology were stagnant. This view is obvious in many books on the history of technology.

It is true that in the Middle Ages science was more or less the handmaid of religion and that natural philosophy in those days was less concerned with experiment and observation than with philosophical argument. The bonds with technology and its practical experience were virtually severed. Hence we should not look for evidence on technological evolution in medieval scientific documents. The evidence is buried in local documents which are rarely studied by the historian of science. Legal, economic and social historians have investigated these documents, manuscripts and letters, each from his own angle, but little technical information has been published nor have illustrations, paintings and drawings been collected.

Therefore our evidence on medieval technology is still scrappy. On the other hand we have by now realised that the fall of the Roman Empire nowhere meant a gap in technical evolution. It is also clear that from the early Middle Ages onwards important discoveries and inventions were added to our material civilisation. Not only was there a constant trade with the Mediterranean and a continuous development of the technological achievements of the ancients. The "barbarian" invaders themselves brought widely diverse gifts to Western Europe such as the use of furs and trousers, a new type of house better suited to the climate than the old patio house,

* Professor, Municipal University of Amsterdam, Netherlands.

*Centaurus* 1953:3:49–57

felt-making, cloisonné jewelry, skis, the domestic use of soap and butter, production of barrels and tubs, falconry and new economic plants such as oats, rye, spelt and hops. It is not yet fully known which of these elements are indigenous and which may have been derived from Arab civilisation or even further East by the way of the steppes.

Last but not least new spiritual forces were at work which changed the development of science and technology profoundly. The western world was a Christian world. It held that man was created God's image and that all souls were equal to God. Man should never be degraded to become a machine. As the centuries flowed by this moral tenet became a strong force destroying and counteracting the use of human slaves as a source of energy. It stimulated the creation of machinery to help mankind and to bring man greater comfort. God had placed nature at man's disposal. Men did not only begin to dream of machinery to take over their heavy duties; they built them—and even strove to make fully automatic machinery, of which the ancients had never even dreamt.

This rise of mechanical power sources was of the utmost importance for the mass-production of metals. A three-fold development was involved. First of all there was the improved harnessing of horses introduced in the ninth century, which allowed the horse to pull loads efficiently and turned it into an economic source of traction.

Secondly the wind-mill came to the West, but as it found its way mainly to the low-lying windy plains along the Atlantic, it hardly plays a part in the development of metallurgy.

The third development, however, that of the water-wheel, was a turning point in the production of metals. It had been introduced from Pontus into the Mediterranean world in the first century B.C. and classical authors like Vitruvius and Pliny refer to the "hydraletes". It is well-known that water-wheels were used in Antiquity. The mills built by Trajan on the Janiculus played a part in the flour-production of Rome during the fourth to seventh centuries. We know of floating water-wheels on the river Tiber. A water driven saw-mill is mentioned in the third century and others were used for fulling and pressing olives.

Yet this form of mechanical power was never popular in Antiquity. The rivers of the Mediterranean world flowed too irregularly to provide a constant supply of water to the wheels. But above all there was no social urge to replace the fairly abundant human power by machinery, no trend to economise on human energy. Vespasian refused to allow the use of a water-driven hoist "lest the poor have no work". When Constantine the

Great adds the flour-mills to the places of penal servitude this means that the majority of flour-mills were still hand-driven.

Already in the second century A.D. we find water-driven flour-mills working for the Roman army at Tournus (Bourgogne) and at Barbegal near Arles a double set of eight water-wheels built cascade-wise ground flour rather inefficiently. Their use spread rapidly in Gaul. In the fourth century two mills are mentioned at Dyon and Geneva, in the sixth we hear of six, but many more references date from Frankish times. By the eighth century water-wheels are well established in Central Europe (Thuringia, Odenwald, Mühlhausen).

Cassiodor explains that the sites of monasteries was often chosen to provide ample water power. The first water-wheel in England is mentioned in a document of 838 A.D.; in the tenth century they have penetrated to Ireland. Water-wheels are quite common in the *Capitulare de villis* and the Domesday Book. By the twelfth century they have spread to Scandinavia and the Baltic and around 1200 they appear in Iceland.

However, the water-wheel was not to remain merely a mechanical means of grinding corn. It soon became the principal source of mechanical power of the Western World and, with the windmill, held its own until well into the Industrial Revolution. Both undershot and (the slightly later) overshot wheels were used by the Egyptians for the supply of water to the fields as Strabo reports. In Western Europe the Cistercian monks used mostly wind-mills for the drainage of fens and lakes.

In the higher regions water-wheels became a very important factor in mining and metallurgy. They were used to drive hoists. By the twelfth century they were introduced in the coppermines of the Harz mountains and the silver mines of Trient. Water-driven hammers for crushing ores are mentioned in Styria around 1175. Waterdriven forge-hammers were common in the thirteenth century. In the first half of the fourteenth century water-power was used in wire-drawing. Grindstones were water-driven in the Wupper valley since the thirteenth century.

This general application of the undershot water-wheel in the Middle Ages enabled the medieval metallurgists to make larger and heavier metal objects. From the twelfth century onwards water-driven bellows supplied a much larger quantity of air to the larger furnaces then built. We shall revert to this when discussing the new cast-iron metallurgy.

The manifold application of the water-wheel and its gearing to other machinery led to much practical experience and theoretical interest in cams, gears and other aspects of mechanics. This is quite obvious when

the first printed books begin to show the achievements of Leonardo da Vinci's contemporaries, who must have based their machinery on the experience of earlier generations. It includes the common use of the crank, one of those inventions that seem to go back to the early Middle Ages.

Again we should not be surprised if further research reveals a connection between this gradually increasing practical experience with cams, gears and machinery and the theoretical considerations by Nicolas Oresme and his generation. The rise of machinery cannot have failed to influence the rising science of mechanics. Both Grosseteste and Roger Bacon express interest in machinery.

The water-wheel, therefore, forms the first and principal factor in the development of medieval metallurgy by providing more powerful sources of blast-air and mechanical means of working and shaping larger pieces of metal. The other factors in metallurgy,—mining ores, producing fuel, and metallurgical furnaces, must now be discussed.

We know far too little about medieval mining methods. Most of our evidence has been culled from the writings of legal historians and local antiquaries, who very often approach their documentary evidence from an angle differing widely from that of the historian of technology. But we are sure that Roman traditions were never lost. New departures were attempted which received a severe set-back at the time of the Black Death and again after the discovery of the New World when many European silver and lead mines were closed for ever. This is quite clear from the Bergbüchlein of the sixteenth century and more especially from Agricola, whose references to ancient classical practice are very frequent. The Harz, Saxony and Bohemia were, however, the best mining schools of Europe long before Agricola's days.

Mining was still limited to shallow shafts which could not reach beyond the subsoil water. The advent of proper machinery, pumps for draining the mines and mechanical fans for proper ventilation, would open up a new area. But even the simple forms of hoists, chain-and-bucket pumps and other machinery driven by man- or horse-power cost money. Mines, no longer state enterprises, were worked by miners grouped in voluntary associations. As the machinery became more costly and larger undertakings were demanded they were financed by the bankers of early capitalism. The Fuggers, the Welsers, the Thurzos and their French and Italian colleagues were deeply involved in the mining and production of metals.

We have very little information on the rare attempts to use coal for

the production of metals from their ores. Coal was mined in the Liège district before 1198. In Newcastle the coal-miners obtained confirmation of earlier privileges in 1234. In Germany the Saar coalmines were producing in the fourteenth century which also saw an extension of this production in the Liège district, Scotland and England. The "sea-coal" trade with its special "keels", flat bottomed boats of shallow draught, became fairly important, but this coal seems to have been used for domestic consumption only.

The metallurgical fuel par excellence was charcoal which was still produced in the old classical way. Grave difficulties loomed ahead when the demand for metals rose considerably. This was due to the rise of the cities in the twelfth and thirteenth centuries and their growing engineering problems, such as the building of churches, housing, harbours, moles, canals and other public works and the rise of modern warfare involving fire-arms and guns. The shortage of timber made itself felt in many districts and led to restriction and even to the cessation of metallurgy in certain regions. Thus in England the shortage of timber meant the end of metallurgy in the Weald. The pressing need for other fuel eventually led to attempts to use coal for metallurgy, but these fall in a later period. The final solutions was reached through the experiments of the Darbys in the eighteenth century. Thus medieval metallurgy remained a powerful factor in the deforestation of certain regions of Europe.

European metallurgy was now centred in the production of iron. Already in classical times the Iberians and Gauls were proficient iron smiths and the production of bronze weapons and tools declined from the second century B.C. onwards. The Celtic smiths of Gaul produced their famous swords by welding steel strips onto strips of wrought iron, but these swords bent easily in use and had to be straightened out. Here again, classical tradition was never lost. The strong guilds of smiths of the Roman Empire survived the storms and their position was strengthened by local statutes. Further specialisation is shown in the rise of the farriers, white-wrights, gunsmiths, pewterers and other guilds.

A great variety of processes and furnaces were used. The primitive bloomeries, producing blooms of 60–70 kgrs., survived up to the nineteenth century. Corsican and Catalan forges were used and made more efficient in certain districts by the introduction of blast air under pressure produced by falling water, a practice that seems to have arisen in medieval Italy. Osemund furnaces, forerunners of the true blast furnaces, produced about 5–6 charges of 15–20 kgrs. of wrought iron a day. Steel was

manufactured by widely different processes. In Styria iron ores containing manganese compounds were reduced at high temperatures, absorbing carbon. Other districts such as Norway and Brescia (Italy) used case hardening processes or even decarbonised in small quantities.

The most famous centres were Styria, Carinthia (producing the "*lymbriquestuff*" and "*iebrookstuff*" of the English market), Tyrol, Amberg, the Harz, Norway, Siegen, Liège (also famous for its brass or "dinanderie"), Spain, Normandy, the Weald, the Mendips, the Forest of Dean and Rockingham forest.

The most important progress in medieval metallurgy was the commercial production of cast-iron. Potentially this manufacture was possible in the furnaces then known, given the proper processing time and temperature of the charge. In classical times cast-iron had been produced accidentally but mostly rejected because the technique of its processing was still unknown. As the furnaces increased in size, due to the trend of producing larger charges more efficiently, the frequency with which cast-iron was obtained accidentally increased. However, the temperatures common in early medieval furnaces were not sufficient for commercial production of cast-iron. In general the blast air was still supplied by bellows which were at best powered by man- or horse-driven treadles. The turning point was the coming of water-driven bellows. Only then were the size of the furnace (that is processing time) and the amount of blast air (that is temperature in the furnace) both sufficient for commercial production of cast-iron.

This production of cast-iron was still primitive and was perfected haltingly and gropingly. The true "*Gusz aus dem Erz*"—that is direct production from the ore in a real blast furnace—seems to date from the early fifteenth century. The fining of this crude cast iron in order to reduce the carbon content of the rough brittle product to the desired amount is an accomplishment of the sixteenth century. But the first primitive blast-furnaces go back to the early fourteenth century. They are reported in the Liège region in 1340 and quickly spread to the Lower Rhine and to Sussex, and in 1360 they appear in Sweden. In the same century we hear of the first cast-iron specialist, Merckeln Gast of Frankfort, who casts guns.

The production of cast-iron was stimulated by several trends and inventions. First of all there was the rise of military engineering. The invention of gunpowder goes back to the late thirteenth century. Attempts to make fire-arms seem to have been undertaken on the Lower Rhine about 1325. This technique soon spread to Italy and France. The first attempts used wrought iron, for instance in the form of strips held together

by bands of iron. These early guns fired bronze or stone cannon-balls. Though breechloading was attempted at an early date, the technique of producing gas tight breech blocks was still impossible because metal surfaces could not yet be finished with the required precision. Hence muzzle-loading was the common system.

Soon these firearms were produced in cast bronze. This was a technique that had grown up with the use of bells in church towers. From the fifth century onwards it had been perfected and many generations since Theophilus had added small improvements. The hammering and drilling of proper guns were no mysteries for the bronze-smiths of those days. They gradually adapted their technique to the new requirements of the firearm proper. Even a primitive form of "rifling" was known.

The new material, cast-iron, seemed very suitable for these applications. From 1325 onwards it was more widely used, gradually displacing bronze. The stone cannon-balls disappeared gradually and were replaced by cast-iron ones. This use of metals for fire-arms introduced a potent new factor into technology. It soon became clear that the technique of producing individual guns with their own series of cannon-balls or bullets was most inefficient. Hence standardisation of fire-arms was taken up in many places, culminating in the standard ordnance propagated by the famous artillery schools of Venice and Burgos in the early sixteenth century. This tendency to standardise parts of machines and tools spread to the dockyards and navies. It proved a powerful stimulant towards the manufacture of precision tools, parts of machinery and the machinery for their production.

Standardisation of parts led to a closer study of finishing processes in metallurgy. The great experience of medieval smiths in welding, chasing and embossing, hammering and grinding were put to use in many fields. Needles, nails, forks, scissors, shears, thimbles and files were already produced by specialists and standardisation set in quite early. Wire-drawing seems to have been invented in the eleventh century. The production of the steel for wire-drawing was now undertaken in various centres and the application of water-power to this branch of metallurgy dates from the early fourteenth century.

Metallurgy in general applied man- or animal-driven treadles (the English "olivers") more liberally and with the advent of water-power mass-production of metallurgical objects was ensured. The new material, cast-iron, soon served to produce mortars and cannons and their ammunition, anvils, cooking utensils, and irons, fire-backs and graveslabs.

Sheet metal was already produced and rods manufactured from these sheets by shearing and slitting. This higher metallurgical skill is probably one of the factors contributing to the rise of mechanical clocks in the later thirteenth century. Here well-designed parts, such as the foliot balance (a simple form of escapement of the early fourteenth century) and gearing, made possible the requisite degree of precision and hence the utility of these mechanical weight-driven clocks. Thus metallurgy contributed to one of the instruments which was to help to build the science of the seventeenth century.

The great importance of military engineering for metallurgy is clear from the "*Feuerwerks-* and *Kriegs-bücher*" (such as the illustrated manuscript of Konrad Kyesser, 1395) a series which culminates in the books produced by Biringuccio and Agricola. Its main influence, apart from the factor of standardisation, is to be seen in a series of small inventions. This is logical as the basic metallurgical processes, both physical and chemical, could not yet guide the metallurgist. The gradual development of metallurgy and pyrotechnics stimulated scientific interest in the basic processes. It was definitely helped by the art of assaying which formed the basic control of practical metallurgy.

It is often insufficiently recognised that this art of assaying was real quantitative analytical chemistry, with tools and balances enabling the metallurgist to obtain a fair precision in the analysis of his ores and products. Though here again the basic chemistry of the different tests was not yet understood it did create a sense of quantitative relations between certain chemical compounds which established a body of knowledge for later generations. These assaying results form the most tangible chemical results of the practical metallurgists and the more theoretical alchemists of the Middle Ages. Assaying is also partly responsible for the use and production of mineral acids in the Middle Ages.

Newton, when writing his *Principia* to establish the laws of the macrocosmos many generations later, was also interested in finding similar laws for the microcosmos. Therefore he turned to the notebooks of the alchemists and, as we are told by his assistant Humphrey Newton, "when he kept his furnaces smoking during all summer he repeated the tests and experiments with antimony and an old mouldy book called Agricola de Metallis lay on his table". It was a fitting tribute to the ancient metallurgists that a truly scientific mind like Newton should first turn to assaying experiments to test the laws of the structure of matter.

The great demand for bronze and iron, the scarcity of fuel and the

gradually increasing difficulty of draining the mines combined to make tin an ever scarcer metal until the new resources in the New World and the Far East were tapped. As tin and pewter had been the most common materials for household vessels, this made it necessary to find a cheaper material for these purposes. Here metallurgy stimulated the rise of the glass industry, which from the fourteenth century onwards began to produce glass vessels for common use. At the same time the more general use of pottery stimulated this art to adopt new tin and other glazes and new forms more suitable for common use. Here again progress was slow but constant as the potters and glaziers groped towards better techniques experimentally.

Metallurgy, therefore, stimulated by many factors and producing new metals, amongst which cast-iron was foremost, gathered a body of technical, mechanical and chemical (assaying) knowledge for future generations. It stimulated and was affected by standardisation of its products, better finishing techniques and new fields of application such as military engineering. The application of water-power made most of these things possible, it was itself the outcome of a long standing desire for mechanisation and, apart from experimentally building up engineering technique, it also led, (along with ballistics) to the theoretical study of mechanics and mathematics.

# CAUSE & EFFECT IN THE HISTORY OF SCIENCE

by

S. Lilley*

Any historical study must necessarily pass through two stages. In the first events are chronicled—the important point is to discover exactly what happened, in a descriptive sense, and exactly when. When sufficient chronicling has been done, the second stage is reached—the problem now is to establish causal relationships between events, to come to understand *why* things happened as they did.

While the chronicle aspects of the history of science have not been (and never will be) exhausted, sufficient fact has been accumulated for historians of science to pass on to the second stage, to try to discover the general laws of cause and effect operating within their field. If we desire to use history as a guide to the future, as many of us do, then the study of causal laws becomes all important. For a generation or so much attention has been devoted to these questions, and many important results have been attained. The most notable result, however, has been the division of historians as a whole into two opposing camps:

(a) those who stress mainly internal causes or influences—the comparatively high degree of rational order in the development of science and the mechanisms through which that order is achieved;

(b) those who direct attention mainly to external influences—motives arising from social needs and desires, methods and modes of approach derived from the general framework of thought current in a society, experimental techniques made possible by technological development, etc.

* M. Sc., Ph. D., Resident Tutor, University of Birmingham, Department of Extra-Mural Studies.

*Centaurus* 1953: 3: 58–72

The general tendency has been to regard these two modes of causal explanation as mutually exclusive alternatives. The purpose of this paper is to suggest that, on the contrary, they are mutually complementary—that the development of science can be fully understood only if the internal and external types of influences are considered together and in their mutual interaction.

Any scientific development, I suggest, becomes possible only when both internal and external conditions are ripe. So much can be demonstrated by considering conspicuous cases in which, for a considerable period, one set of conditions, *either* external *or* internal, was favourable to an advance, but the other was not.

It is perhaps merely stating the obvious to say that a scientific advance can never be achieved, merely in response to motives of social needs no matter how strong, or merely as a result of research opportunities provided by a society which has those motives, unless the earlier progress of scientific knowledge and technique has also prepared a base from which the new exploration can begin. The records of the Royal Society in the 17th century are full of schemes, socially desirable and actively pursued, that failed for this reason only. Men have desired an effective science of healing for millenia, but only in the last century or so has the advance of biology made this attainable.

But conversely—and this point is less generally appreciated—the mere preparation of all the internal prerequisites for a new scientific advance does not guarantee that that advance will be made. If the general social outlook—in which all scientists share to a greater or less degree—regards a certain line of work as unimportant (or even dangerous) and so discourages men from working on it, denies them opportunities, or sneers at their efforts, then few (if any) will work on it, and the chances of success will be small. And if, by chance, some lone worker, partially isolated from the general sentiments of the community he lives in, does in fact work on the subject and produce some advance in it, then his work is ignored. He becomes one of those "precursors", who are intriguing figures in the history of science, but whose influence on the main line of development is small, or is delayed for centuries. That was the fate of Peter Peregrinus, who in the 13th century made many important discoveries in magnetism. Society in his time had no great use for magnetical knowledge, and his work was without any important influence until the end of the 16th century, when the rapid expansion of navigation put a premium on the science of magnetism.

Earlier studies of social influences on the development of science tended to concentrate rather exclusively on questions of motive. Economic needs—for example, for improved navigation in the 17th century—created demands for scientific knowledge on which to found a new technology. Scientists responded to the social demand—some consciously, others merely absorbing unconsciously the atmosphere in which they lived. And hence the social demands led to corresponding scientific developments.

More recent studies have tended to modify this approach. Needs and demands of society are still seen as important factors in controlling the progress of science. But it has also been realised that social development acts on science in other and more subtle ways. For example, general social conditions impose on scientists habits of thought of which they are not conscious— habits which are determined not so much by existing scientific knowledge as by modes of thought and action that are typical of the society in which they move. At one stage these habits may prevent the scientist from reaching an appropriate interpretation of the evidence around him. At another stage, when social changes have induced new habits of thought, there comes a flash of enlightenment and evidence that has been available for years is suddenly reinterpreted to give a completely new scientific synthesis.

Such a case is the discovery of the conservation of energy. Virtually all the scientific pre-requisites for this discovery were in existence by 1800. Much of the evidence was actually in print. The rest could have been obtained by experimental techniques well within the capacity of the time; it would have been obtained almost automatically if the current habits of thought had not prevented physicists from asking the right questions about perfectly familiar phenomena. And yet the discovery of the conservation of energy was not made till nearly 50 years later. In the 1840's several men—Mayer, Joule, Colding, Helmholtz and others—arrived independently and almost simultaneously at the new concept. The way in which they did so does not suggest that a piling up of new evidence was the essential cause. Though one must not neglect Joule's painstaking experimental work, one gains the impression from the writings of all these men that they came to regard the conservation of energy as something that was almost obvious *a priori*. In the lecture of 1847 that contains his first general statement of the principle, Joule argued that energy ("living force") is so important in nature "that it would be absurd to suppose that it can be destroyed ... We might reason *a priori* that such absolute destruction" of kinetic energy "cannot possibly take place ..."[1] A few years

earlier nobody had thought of the conservation of energy; now it was obvious—so much had the habit of thought changed.

And when one examines closely the work of Mayer, Joule and the others, one discovers that the essential novelty in it was not an accumulation of new evidence, though some new evidence had arisen[2], but a new attitude of mind, a new habit of thought—an intense interest in those phenomena which we can retrospectively describe as quantitative aspects of energy transformations, an interest that does not appear before the 1840's[3]. Joule accumulated much experimental evidence, yet on reading his work chronologically one sees that the new outlook led him to seek the evidence much more than the evidence led him to the new outlook. And the other pioneers reached their conclusions on almost trivial amounts of experimental evidence—for them the new attitude of mind was all important.

Seeking a source for the new attitude, it becomes clear that it was essentially an importation into physics of the habits of mind of the large-scale factory industrialists to whom quantitative aspects of energy changes, in the form of financial aspects of power production, were of supreme importance. Motives—for example, the desire for improved prime movers—played a part in this development; that was where Joule started from. But far more important was the fact that these men thought in a different way from the orthodox physicists, and that their new mode of thought was a reflection of the every-day mode of thought of the industrial classes, whose influence was now rapidly growing[4]. In this case, then, the essential factor in a major scientific revolution was not the internal growth of existing science, but the importation into science of habits of thought that were created by the social agency of the Industrial Revolution.

These are extreme cases; and extreme cases, though they can be misleading, are often useful as a first approach to a difficult problem. We can, in fact, distinguish two extremes. In the one, if we survey the history leading up to the scientific development under consideration, we find that the social environment (or the relevant parts of it) have changed but little over a comparatively long period, and the explanation of scientific progress must therefore necessarily be sought in terms of the internal development of science itself. Examples of this extreme occur most frequently in branches of science that have become well established. Physical (dynamical) astronomy, for example, owed a great deal in the 17th century to motives derived from its potential social usefulness as an aid to navigation. But when John Couch Adams and Urbain Jean Joseph

Leverrier applied it in 1845–46 to predict that a new planet would be discovered in a particular place, that social motive was gone. Social institutions by then played the passive role of permitting the continuance of lines of research which had earlier been positively encouraged because of their economic value. And the causal mechanism leading up to Adams' and Leverrier's discovery will be found within science itself— chiefly in the mathematical tools which their predecessors had created. In such cases, the historian looking for causes and influences, would merely expose himself to ridicule if he devoted more than a small fraction of his efforts to examining the social environment of the mid-19th century[5].

At the other extreme—that exemplified by the discovery of the conservation of energy—we find cases in which over a period of time the relevant parts of science have changed but little internally. In 1800 evidence from which this generalisation could be deduced was available, and not much more was available in the 1840's. The internal development of science between 1800 and 1840 will therefore not fully explain the discovery of the conservation of energy. And the historian who really wishes to understand why this principle was discovered at just that time must look for changes in the social environment and their effect on the thoughts of physicists.

But we must not expect to find in history many of these extreme, and therefore simple, cases. In general we shall find that both internal and external influences co-operated to promote any given scientific development.

To illustrate the questions that then arise, let us consider one of the most difficult and important problems in the whole history of science, namely the problem of the origin of the experimental-mathematical method, which appeared in almost perfect form in Galileo and which lay at the root of all 17th century advances in physics and particularly in mechanics. The essentials of this method, in so far as a brief statement can convey them, are: (i) select from the phenomena under discussion aspects that can be treated in quantitative terms; (ii) on the basis of available evidence, which may be derived from special experiments or merely from general observation, formulate a hypothesis which asserts a mathematical relation among the quantities involved; (iii) from this hypothesis deduce mathematically one or more consequences that are within the practical range of experimental verification; (iv) carry out the experiments thus indicated; (v) accept (provisionally) or reject the hypothesis according as the experimental results agree or disagree with the deduced consequences. In Galileo's classical use[6] of the method he wishes to discover the law

underlying the behaviour of falling bodies. His steps, accordingly, are: (i) isolate for consideration only the two quantities, distance travelled and time, and such dependent quantities as velocity and acceleration (e.g., ignore the density of the body, its colour, etc.); (ii) formulate the hypothesis that the acceleration is constant; (iii) deduce mathematically that if this is so, then the distance travelled by a ball rolling down an inclined plane will be proportional to the square of the time travelled; (iv) experiment with balls rolling down inclined planes; and (v) finding that they do behave as predicted, accept the hypothesis.

In Galileo's usage the mathematical element of this method were of extreme importance, but they do not, of course, lie at the core of the method. The success of modern science in general depends on the use of the method in a more generalised form which does not always include mathematical argument—form a hypothesis (which may or may not be quantitative), deduce consequences from it (mathematically or by logical argument), compare the consequences with experiment, and accept or reject the hypothesis according as the deductions and experimental results agree or disagree. In this more general form the method actually gave important results in the work of William Harvey on the circulation of the blood before the appearance of Galileo's main work. And we can learn much by asking first of all how the method in this wider sense came into being.

The reader who is not well versed in the historical development of science may feel that this method of procedure is so obvious that there is nothing to explain. In fact, it was not used—except fitfully and without full realisation of its import—till the time of Harvey and Galileo. So much has the method of science become a part of our normal thought that it requires an effort of historical imagination to realise that before the 17th century this method was very far from obvious. The method that preceded it—that of Aristotle—was rather like the modern one, but with the vital last step ommitted. From observation, often rather casual, one formed by a sort of intuition a generalisation about fundamental causes; and from this generalisation one deduced logical consequences. But now the consequences were regarded as *true*—they did not have to be checked by experiment or observation; nor did Aristotle conceive that such a check would also be a test of the generalisation as such. In this brief description, the method of Aristotle has been somewhat caricatured, but not essentially falsified. Aristotle's teaching was the basis of medieval thought and for the most part his method was simply accepted without serious question.

Occasional workers transcended it in one way or another, but no alternative method really took its place until the time of Harvey and Galileo.

On examining the literature we find that there are two distinct schools of historians who offer two very different explanations as to how the method of Galileo—hypothesis, deduction, experimental verification—came into existence. The first stresses the continuity of development from 13th and 14th century scholastics, who started from Aristotle's position, to Galileo and his contemporaries, and seeks to show that a process of discussion and criticism, mainly within the scholastic tradition, continuously developed the Aristotelian method into that of modern physics[7]. Randall[8] guides us through a series of discussion on methodology which took place in the University of Padua between the 14th and the 16th centuries and which led to a theory of method which approximates (except in regard to mathematics) to that eventually practised by Galileo. Expressed as they are in the highly technical scholastic langugage, the details of the discussion are not suitable for reproduction here. But the upshot was that the Paduans reached the idea that all knowledge must start from empirical observations; that on the basis of these observations, by analysis, by arguing backwards as it were to discover their essential content, one must formulate a hypothesis about the fundamental causes of the observed phenomena; and finally one must argue forward again and show by deduction that the actual observed phenomena are consequences of the hypothesis. Clearly the Paduans had gone a long way towards Galileo's method.

Now Galileo spent many years as a Professor at Padua. In describing his method he used the highly technical language of the Paduan methodologists. His achievement must certainly have owed something to what he learnt from his Paduan predecessors. William Harvey also studied at Padua. He makes no explicit reference to methodology in his work, but again it is reasonable to suppose that his successful application of the method in its non-quantitative form owes something to what he learnt at Padua.

But how much did these two gain from this source? Enough to explain their achievements without reference to other sources? I think not. The best statement of the Paduan methodologists falls considerably short of Galileo's practice. The material that Randall presents does not seem to me to show that the Paduans were fully aware that the agreement or disagreement of their deductions with empirical fact was the ultimate test of the hypothesis. Nor do they suggest—at least in the passages quoted by

Randall—that one ought to deduce predictions about new phenomena which were not used in formulating the hypothesis, test these predictions experimentally, and thus avoid the danger of that sort of unconscious 'cheating' in which one puts into a hypothesis everything that one hopes to get out of it. Ultimately the test of a hypothesis is not whether it will explain the empirical facts that originally suggested it (for hypotheses that will do that can be created *ad lib.*), but whether *other* deductions made from it also satisfy the test of experience; and this point does not appear in the Paduan statements, whereas Galileo appreciates it in practice and almost states it explicitly[9]. And finally, there is little evidence that the Paduans did put the method into practice—that they did make a habit of testing hypotheses by deducing consequences and comparing these with experiment. There is thus a considerable gap to be filled between the best products of the Paduan methodological discussion and the methods which Galileo and Harvey actually practiced. It is at least plausible that other factors, besides the Paduan theories, have to be taken into account before the genesis of Galileo's method can be fully explained.

Let us now turn to the other answer to this problem, most ably propounded by Zilsel[10], which sees the origin of the new method largely in terms of social changes external to science. The development which Zilsel traces is of a very different kind—not a story of scholarly discussions about how one *should* investigate, but a tale of how more lowly men *did* act in practice. The emphasis at the beginning is on the actual practice of experimentation rather than on the theory of how experiment should be used. Experiment according to this view first became common among craftsmen. The craftsman's routine activities are in a crude sense experimental. His habitual test of "truth" is empirical—does the device work or not? The social changes of the 15th and 16th centuries, the great increase in wealth and the expansion of industry and commerce gave wide opportunities for ambitious craftsmen to "get on" by improving their crafts, and at the same time threw up superior classes of craftsmen, who became differentiated from the masses—instrument-makers, surveyors, navigators, gunners, surgeons, and above all the artist-engineers, of whom Leonardo da Vinci was the supreme example. These men sought to improve techniques, and as a natural consequence to increase knowledge and some of them passed beyond the desire for knowledge merely as an aid to practical improvement. In doing these things, they naturally used an extension of the craftsmen's ordinary activities—that is to say, they experimented. It certainly seems to be true that frequent recourse to

experiment is to be found earlier among superior craftsmen than among scholars. And most of the men who in the 16th century came closest to the empirical and experimental spirit of modern science belonged basically to the class of superior craftsmen, or were at least nearer in social position to the craftsmen than to the University scholars or the Humanists—Leonardo da Vinci (civil, mechanical and military engineer, architect, artist), Ambroise Paré (surgeon), Robert Norman (compass maker), Biringuccio (author of the first published book on metallurgy), Dürer (artist and military engineer), Stevin (book-keeper, mathematical tutor, military engineer and quartermaster-general), Tartaglia (mathematical teacher, chiefly for practical men, and ballistics expert), and many others. In our outline histories of science, we lump these men together with such scholars as Copernicus and label them all "scientists". But it is important to realise that while Copernicus was educated primarily as a learned man, these others whom we have named were essentially practical men who picked up their learning as an auxiliary aid to their practical activities[11].

These men—craftsmen or socially their near relations—were the first to make habitual use of experiment in scientific investigation. But for all that, they did not achieve the experimental *method*—by which we mean, not merely experimentation however frequent, but the use of experiment in the way I have indicated to verify or reject hypotheses. Though many of them had a great thirst for knowledge and used their enhanced social position to acquire a vast amount of the scientific learning that had been inherited in the scholastic tradition, yet they did not have enough rational and systematic training to enable them to develop their *habit* of experimentation into an experimental *method*. Leonardo, to choose the best of them, constantly determined facts or even simple relations between them by experiment; he constantly deduced consequences from hypotheses. But he did not reach a proper combination of these two processes—he did not systematically employ the comparison of deductions with experiment as a test of hypotheses[12]. In a word, though these superior craftsmen had the very praiseworthy habit of experimenting, they did not—could not—learn how to use experiment as part of a method for systematically testing hypotheses.

This last step, according to Zilsel, arose from a further social trend. The increasing importance of industry, and particularly the rise of capitalist industry, which brought 'gentlemen' into a closer relation with production, led certain scholars in the later 16th and 17th centuries to take an interest in craft matters. Thence they learnt the value of experiment, and also

learnt that it could be valuable even in cases where improvement of a craft was not the main objective. At the culmination of this process stand men like William Gilbert and especially Galileo. The close contact between these two and the craftsmen is unambiguously attested. Gilbert's *De Magnete* (1600) is full of references to the crafts and some of his major achievements were derived from the work of the compass maker, Robert Norman. Galileo set up his own workshop at Padua for the production of scientific instruments for the market, and employed artisans in it for wages. His researches included many craft matters—pumps, fortresses, military instruments and the like. He employed craftsmen to aid his researches and constantly visited others in their places of work. His chief work, in which the new method first fully appears, is written in dialogue form with the scene set in the Arsenal of Venice, and in it[13] he unashamedly declared that he could and would learn from craftsmen. From that source, then, Gilbert and Galileo learned to regard experiment as the ultimate test of truth and to *act* accordingly. But they did more—they combined the practice of experiment with their own systematic, scholarly training, and so produced the modern method. In Gilbert the process is very far from complete. He experiments as much as any craftsman, but shows little trace of a systematic use of experiment to verify hypotheses. At last, with Galileo the marriage of craft practice and scholarly theory is consummated, and from it is born the method of modern mechanics—hypothesis, deduction, and experimental verification.

Now this explanation again tells us much that we want to know. Many generations of scholars and philosophers—at Padua and elsewhere—had theoretically admitted that the ultimate source of truth was empirical observation and preferably controlled experiment. But their actions in general belied their words. Why then in the 17th century should they suddenly start to experiment in practice? Zilsel's answer that they really learnt the value of experiment from the craftsmen—and not from theorising about method—is a very plausible one. Equally this theory explains how it could happen that the non-scholarly types of scientists could experiment busily for a century without reaching the experimental method, and why the proper way of using experiment could only be discovered by men who belonged to the formerly non-experimental learned tradition. All this is explained very reasonably in terms of social changes.

Yet this view, too, has its weaknesses. The jump from realising the value of experiment to learning how to use experiment in the full process of hypothesis, deduction and verification is a big one. It cannot be

dismissed merely by saying that once scholars learned the value of experiment they would automatically see how to use it systematically.

So we have two theories about the source of Galileo's method: one in terms of the internal development of scholastic science, the other in terms of social changes. Each, we have seen, explains a good deal; each leaves something unexplained. Those who argue at length that the one or the other of these theories is correct are clearly wasting their breath. Neither is sufficient. Nor is it much more sensible to ask which theory comes nearer to the truth. The most likely road to a solution of our problem is to abandon the practice of isolating internal or external causes and trying to prove that one or other alone explains the whole progress of science. Instead the two processes—the internal methodologising and the external social changes—should be considered together. The really fruitful questions are not "Which theory is correct?" or "Which is more correct?", but "How were these two developments related? Where, when, and how did they interact with one another, and what new steps towards the experimental method arose from each interaction? How did the two traditions get blended together and integrated, and what new features emerged as a result?".

Here it is necessary to confess that we can do little more than pose these questions. Little has been done towards answering them. We can see, of course, that the two processes come together in Galileo. He was heir to the Paduan tradition[14] and he maintained, and learned from, close contacts with craftsmen and their problems. And when we think of Galileo as a man who theorised about procedure as his Paduan predecessors had done. but had also a deep conviction, derived from craft contacts, of the value of experimental test, we *begin* to see why it was in him that the full method should have appeared. But we only *begin* to see —Galileo's whole life and work would have to be studied anew in order to see concretely how in his person the two traditions were blended.

In any case is this the only interaction of the two trends that must be taken into account? Did they simply develop independently for a century or so, to be united only at the end of their development? Granted that only in the 17th century would social conditions allow the consummation of the marriage between scholar and craftsman, yet before the marriage there must have been a courtship and an engagement. I feel sure that when we know the whole story we shall find that at many points in the 15th and 16th centuries the internal scholastic development came into contact with the busy experimenting craftsmen; and at every contact, we shall discover, a new step was taken in the evolution of the method. Clearly

there is such a contact in Leonardo. He was one of the chief transmitters of the best scholastic tradition to the moderns and also the greatest of the 'artist-engineers'. Though much has been written on him, he would probably repay a new study directed to discovering in what way his thoughts arose from a *synthesis* of the two trends.

There must be many more points of contact between the two traditions. But we do not know of them, or rather we often know they existed, but do not know if and how they affected the development of method. By and large these two major lines of development have been studied by different schools of historians who have kept their studies so much apart that the relations and interactions between the two movements have been almost entirely neglected. Though Randall has recognised that the development at Padua owes something to the fact that logic was studied there as a preparation for medicine instead of theology or law, yet we do not know, for example, if it was in any way influenced by the rising status of the *craft* surgeons as contrasted with the *learned* physicians. And so one can do no more than *pose* the question of studying the interactions between the scholastic development and the craft development with a view to discovering whether a synthesis of the two may provide a satisfactory explanation of the causes leading up to the method of Galileo—pose it as one of the major questions to be answered by this generation of historians of science.

I do not mean to imply, of course, that these two are the only trends to be considered; a complete explanation of the origins of Galileo's method will probably require a synthesis of these with several other approaches. In fact several more approaches are introduced immediately we begin to consider the mathematical aspects of Galileo's method. The more or less internal trends that would have to be taken into account in explaining fully this mathematical side include, for example: continuous developments deriving from 14th century scholastics[15]; the effect of the rediscovery of certain Greek mathematicians, notably Archimedes; the influence of astronomy, in which mathematics had been used for millenia; changes in metaphysical outlook, induced perhaps by the neo-Pythagorean and neo-Platonic philosophies of the Renaissance, which led scientists to regard the universe as essentially a mathematical design[16]. Some objection can be raised against each one of these, at least as a complete explanation—Duhem's claims, in the view of later critics, are exaggerated; to rediscover Greek mathematics, even as used by Archimedes, is still a long way from learning how to use mathematics in conjunction with empirical data as

Galileo did; as regards the influence of astronomy, it is noteworthy that Galileo himself was almost entirely qualitative in method when he dealt with that science, so that it is hard to believe that this is the source of his mathematical physics; and neo-Platonic and neo-Pythagorean metaphysics tended in general to lead to numerological mysticism rather than to mathematical physics. Turning to possible social causes, Strong[17] presents a good, though far from perfect, argument for the thesis that the habitual use of mathematics in physical science (as distinct from astronomy) began among craftsmen (surveyors, ballisticians and the like), and that the full integration of hypothesis, mathematical deduction and experimental test came about by the same synthesis of craft and scholarly traditions as has already been described in relation to experiment. And beyond this is the further possibility, which has not yet been seriously investigated, that the mathematisation of the universe in the 16th and 17th centuries (and with it, perhaps, the whole of the new metaphysic) was actually a reflection within science of a new way of thinking in society as a whole that arose from the changeover to a mercantile capitalist economy—that, to put it crudely, when the physicist reduced all qualities to quantities or the philosopher asserted that a mathematical reality underlay all appearance, they were really following in the trail of the merchant who reduced all his goods to quantities (of money) irrespective of their qualitative differences.

Here again, I suggest that our problem of the origin of Galileo's mathematical physics will not be solved by arguing that one or other of these ideas embraces the whole truth. Each may hold part of the truth; certainly none holds it all. It is only by considering the problem synthetically, by taking account of all these trends of development and especially of the interactions among them, that we shall really come to understand the causal mechanism that led to Galileo's achievement.

So my discussion of this problem has led to very little in the way of a positive answer. But it does, I think, point clearly to the path we must follow if we are to solve this problem in particular or more generally if we are to make progress in understanding the causal mechanism that lies behind scientific development. We must stop asking, "Are internal or external influences *the* causes of a scientific development?" We must seldom allow ourselves to ask "Are internal or external influences more important?" We must concentrate attention on the question "How have internal and external influences interacted with one another? Helped or hindered one another (for the tale is not always one of co-operation)? Become integrated and synthesised with one another? At every stage what

new things have emerged from the interaction or synthesis? And how far do these give a satisfactory explanation of the actual course of development of science?"

When we look at the history of science in this way, we find that the distinction between internal and external causes or influences begins to disappear. Powerful causes or influences usually have a dual aspect—partly internal, partly external—they are powerful *precisely because* they have this dual aspect, because two powerful forces are co-operating to produce one that is yet more powerful. The time has come therefore to stop arguing about the relative importance of internal and external influences, and instead to approach the causation of scientific development as a unity in which both aspects are studied, and studied in their mutual interaction.

NOTES

1. J. P. Joule: *Scientific Papers* 1, 268–9.
2. Particularly in electro-chemistry and electro-magnetism. Yet it is very noticeable that the men of the orthodox physical schools who made these discoveries, made little approach to the conservation of energy—their mode of thought did not permit them. The new principle was discovered by men outside the orthodox physical tradition, who entered the field of physics by various unorthodox routes—from engineering (Joule, Colding), from medicine (Mayer, Helmholtz), etc.—and who were thus free from an internal physical tradition which perpetuated the older habits of thought.
3. Save in Sadi Carnot, the negligible effect of whose work till Kelvin took it up merely serves to emphasise that before a new idea can succeed, not only the discoverer, but also a substantial section of society, must be attuned to it.
4. To sustain this thesis in full would require a detailed examination, not only of the works of the discoverers, but also of the relations of these men to the industrial groups in society, of the attitude of the industrialists to science and to their factories, and much else. The gathering of this evidence has occupied much of my time in the last few years, but I have not yet been able to publish it. A preliminary approach will be found in S. Lilley: *Social aspects of the history of science*, Archives Internationales d'Histoire des Sciences, 28 (1949), 376–443, especially 382–3 and 390–401. See also L. Rosenfeld: *La genèse des principes de la thermodynamique*, Bull. de la Soc. Roy. des Sciences de Liège (1941), 199–212, and *Joule's scientific outlook*, Bull. of the British Soc. for the History of Science, 1 (1952), 169-76; J. G. Crowther: *James Prescott Joule, 1818–1889* in his *British Scientists of the Nineteenth Century* (Pelican Edition, Harmondsworth, 1940), 149–220.
5. Even in such cases as this, there still remain many interesting problems concerning the interactions between scientific discoveries and their ambient societies. See Prof. Pannekoek's discussion of the reception of Adams' and Leverrier's predictions in their respective countries (pp. 126–137 of this volume).
6. *Dialogues concerning two new sciences* (1638), English translation by H. Crew and A. de Salvio (New York, 1914), Third Day, pages 153 ff.

7. Since this paper was written, the publication of A. C. Crombie's *Robert Grosseteste and the origins of experimental science* (Oxford, 1953) has thrown new light on medieval contributions to methodology. Crombie shows clearly that several 13th and early 14th century scholastics made notable progress towards the conception of hypothesis-testing, and some practised experimentation sporadically in something like the modern way. But his evidence does not seem to me to justify his claim that these medieval efforts were the sole and complete source of the modern method. In any case the decline of medieval society brought a regression in method. Little experiment was practised by scholars after the first few decades of the 14th century, and even the *theory* of methodology went back towards the Aristotelian position. To prove that 16th century workers used 13th century manuscripts does not establish that they derived their method therefrom—but only that when they were beginning to develop the new science, they sought help in the best earlier works. The only *continuous* connection so far established between the high medieval period and the 16th century is that at Padua; and Randall (see note 8) makes it clear that the Paduans started with a methodology nearer to that of Aristotle than to that of Grosseteste, Bacon, Peter Peregrinus and Theodoric of Freiberg; and even by the 16th century they had not regained all the best medieval achievements. Thus, while enhancing our appreciation of medieval efforts, this book illustrates very well my general thesis that isolating single trends and asserting that they alone matter will not give us a full understanding of causal relations in the development of science.
8. J. H. Randall, Jr.: *The development of scientific method in the school of Padua*, J. Hist. Ideas, 1 (1940), 177–206.
9. *Op. cit.*, English trans., p. 178 & 148–9.
10. E. Zilsel: *The sociological roots of science*, American Journal of Sociology, 47 (1942), 544–62; *The origins of William Gilbert's scientific method*, J. Hist. Ideas, 2 (1941), 1–32.
11. Even though some of them went a step further and began in some measure to enjoy learning that had no immediate practical importance.
12. S. Lilley: *Leonardo da Vinci and the experimental method* in *Atti del Convegno di Studi Vinciani indetto dall' Unione Regionale delle Provincie Toscane* (Accademia di Scienze e Lettere "La Colombaria", Serie dei Volumi di Studi; Florence, Olschki, 1953 – in the press).
13. *Op. cit.*, opening of First Day, pp. 1 ff. of English trans.
14. Gilbert, on the contrary had no connections with the Paduan or any other advanced scholastic methodological tradition. This serves to explain why he, in contrast to Galileo, failed to find a correct relation between theory and experiment. More thoroughly than Galileo he adopted the experimentalism of the craftsman, but for lack of scholarly methodology he could carry it little further.
15. Described by Duhem (e. g., in *Etudes sur Léonard de Vinci*, 3 series, Paris, 1906–13) and his followers. Cf. Randall, *op. cit.*, 181–2 and Crombie, *op. cit.*, 91–127 and *passim*.
16. E. A. Burtt: *The Metaphysical Foundation of Modern Science* (London, 1925). Cf. A. Koyré: *Etudes Galiléennes* (Paris, 1939), and elsewhere.
17. E. W. Strong: *Procedures and Metaphysics* (Berkeley, 1936). Strong, whose object is to refute Burtt (*op. cit.*) and who in denying virtually all metaphysical influence certainly overstates his case, does not greatly emphasise the social aspects of his argument, but I think my statement here is a valid interpretation of his data.

# THE FRENCH REVOLUTION AND THE PROGRESS OF SCIENCE

by

R. Taton*

Like any important historical movement, the French Revolution forms an extremely complex totality. Prepared by a slow evolution of institutions and ideas, it included very diverse successive phases and was followed by very varied periods of regression. Its influence on the progress of science is also rather difficult to state precisely, because as in all periods of turmoil, not only were certain conditions favourable to the progress of science established, but also other forces acted in the opposite direction; and the results of the interaction of these diverse factors were often contradictory. Nevertheless, when the evidence is weighed, the balance remains clearly positive, and we can say that the French Revolution influenced the progress of science very favourably by allowing French science to regain, though very temporarily, the undisputed supremacy which it had lost after its period of splendour in the middle of the 17th century, by laying the solid foundations for a new and valuable system of scientific and technical education, by encouraging scientific research in its various aspects and by enlisting, by its example, the majority of countries in a pacific competition which was propitious to the rapid progress of science. While it is obviously very difficult to discern whether, in a different set of circumstances, a specific discovery would or would not have appeared at the same period, it is nevertheless possible to show how the general direction of French science was essentially conditioned by the new conditions created by the Revolution. And our aim here is to evaluate these new circumstances, to discover their influence on the progress of French science during the closing years of the 18th century, and the opening decades of the 19th, and to trace their more long term

* Chargé de recherches au Centre National de la Recherche Scientifique (Paris).

*Centaurus* 1953: 3: 73–89

repercussions on the evolution of the sciences and techniques as a whole in the course of the 19th century.

We shall begin with a rapid survey of the evolution of scientific life in France up to the Revolution, paying particular attention to the situation in education which in fact conditions the effective diffusion of science and the possibility of its making rapid and durable progress. This preliminary study will allow us to evaluate better the influence of the Revolution when we come to it, after giving some attention to the part played by intellectuals in the revolutionary movement and on the attitude of the new political personnel to science. The main effect will be seen to lie in the democratisation of education and in the creation of a system of higher technical and scientific education, whose great value will be very obvious. We shall also note in passing the important part played by scientists in national defence, the creation of the metric system, and various minor consequences. The majority of these effects and of the innovations introduced by the French Revolution continued, furthermore, to produce fruit all through the 19th century and therefore constitute an important part of the global effect of the Revolution.

But, if it would be profoundly unjust to challenge the importance of the influence of the Revolution of 1789 on the evolution of science, it would be equally vain to see in it only the effect of a sharp break with the immediate past, and to ignore the patient work of the scientists, the philosophers, and the encyclopaedists of the 18th century.

During the first half of the 17th century, science in France had been the almost exclusive privilege of enlightened amateurs, maintaining contact with one another by frequent correspondence, or meeting in private academies. Scientific education was moreover more or less neglected, both in the colleges of the universities and in those attached to various religious orders, and the work of their teachers was confined almost exclusively to the publishing of a few didactic works. Nevertheless, while the universities were bogged down in dead routine, a few professors of the Royal College (*Collège de France*) tried in their teaching to follow as closely as possible the progress of the various sciences and the evolution of the scientific spirit. The foundation of the Royal Academy of Science in 1666, by furnishing a quite substantial material aid to a fair number of scientists in the capital and by creating close contacts between them, undoubtedly helped to improve the general conditions of scientific research. The creation of various learned journals and collections, partly scientific, also contributed to a more rapid rise of science. During this time, the scientific

contribution of the 17th century, so important from the point of view both of the method and of positive additions to knowledge, became gradually more widely appreciated in educated circles; the influence of Descartes triumphed in geometry and physics and the great principles of Cartesianism enjoyed a growing popularity. Meanwhile the creation of infinitesimal calculus and the great discoveries of Newton opened up new horizons which various French scientists accepted only with a certain reluctance.

But with Bayle and Fontenelle, the critical spirit came to the fore; the philosophical ideas of Locke, the great principles and the essential results of Newtonianism, publicised and extolled by many writers or scientists—among them, Voltaire, Emilie du Chastelet and Clairaut—began to create a certain infatuation for science among those who, as a result of their education and their resources, were in a position to enjoy intellectual leisure. While it became fashionable to discuss scientific questions in the salons, and while men of wealth created and maintained scientific "cabinets", the Royal Academies of the Continent, encouraged by the more and more marked support of royalty drew into their orbits all scientists of any importance. The correspondence between scientists of different countries, the prizes awarded by the academies and the more and more regular publication of their transactions maintained a fairly intense international co-operation which was made easier by the fact that there were in practice only two languages of science, Latin and French. But this period of the splendour of enlightened despotism which allowed a considerable number of brilliant scientists, in the first rank of which were Euler, Lagrange and d'Alembert, to give themselves up to their work free from preoccupation with material cares, could not be of long duration.

The continued progress of rationalism and of the observing and critical spirit turned cultivated men little by little towards a profound criticism of political institutions and of the hard living conditions of the vast majority of the population. In spite of its imperfections, the *Encyclopédie* achieved a very great success; in extolling the cult of science and reason, it endeavoured to restore the study of the techniques and that of the most humble trades, and many of its articles emphasised the necessity for drastic political and social reforms. The most eminent members of the Royal Academy of Science were among the principal instigators of this enterprise and one can say that the great majority of the Academy approved their efforts, whether they were directed towards the triumph of rationalism, or whether they aimed more directly at social and political reforms. In

this very active intellectual movement, scientists of lowly origin like Monge, Laplace and Vandermonde mixed with nobles like Condorcet, lawyers like Bochart de Saron or rich tax-farmers like Lavoisier. Moreover, although the royal authority in principle governed all the decisions of the Academy, one senses a rather clear democratic spirit at work there; personal relations do not seem to have been much influenced by differences of social origin, and several well-to-do Academicians like Lavoisier, and Bochart de Saron liberally put their resources and their personal laboratories at the disposal of their less fortunate colleagues.

While the prerevolutionary intellectual movement developed in many other directions, nevertheless science played an essential role in most of them. Thus Voltaire and Jean-Jacques Rousseau extolled in their works the educational value of the exact and observational sciences. Elsewhere, the studies of Montesquieu, Turgot, Condorcet and of various Encyclopaedists attempted to lay down the scientific principles of political economy, thus making obvious the need for a radical transformation of society. The essential role of psychology appeared in the new conceptions of the Scottish School and of the followers of Condillac, while already the materialist outlook showed itself in the study of the relations between mind and matter undertaken by the schools of La Mettrie and Cabanis, and while the labours of various naturalists, anticipating the work of Lamarck, laid down the first elements of the new theories of nature.

All these currents, arising from very diverse inspirations, contributed to the creation of a climate favourable to broad social reforms, to an efflorescence of rationalism and free thought, and to a considerable extension of the role played by science in national life. The reform of public education, the democratisation of education and the extension of scientific studies were among the preoccupations common to all those who hoped for progress.

Certainly some improvements had been made in this field, but there remained an enormous task to accomplish. The only people who in practise could get the benefit of education were the rich and certain privileged people, near neighbours of a religious college, like Laplace at Beaumont-en-Auge or Monge at Beaune. Further, if influenced by the fame of the experiments of Nollet and Franklin, many colleges had installed fairly well-equipped cabinets of physical apparatus, yet their scientific teaching remained notoriously inadequate. The high place given to the ancient languages, philosophy and theology confined the first scientific studies to the terminal class; and even these were often imperilled

by the inadequacy of the teachers. At Paris circumstances were more favourable. The College of Navarre and the College of Mazarin, in particular enjoyed a well merited scientific reputation, and from the chair of hydrodynamics created by Turgot in 1775 Bossut and Monge carried out some fundamental scientific teaching; in addition quite a number of scientists gave private lessons, and the private laboratories belonging to various people of the Court or the higher bourgeousie were opened liberally enough to those who wanted to follow study and research. In higher education, though the College of France kept itself in the front line of progress and adapted itself to new needs by creating successively six new scientific chairs—Astronomy (1768), Mechanics (1773), General Physics (1769), Experimental Physics (1786), Chemistry and Natural History (1774), Natural History (1778)—yet the University refused to follow the march of progress. There were various military colleges attached to the services of war, engineering, artillery, or the navy and founded in the course of the century, which played an essential part in the spread of the new science. Certainly their programmes were very clearly influenced by the destined vocations of their pupils and the direction given to their studies was essentially practical. But their teaching was in general of a high enough standard and these schools had the double merit of diffusing a deeper scientific culture and of allowing a number of talented scientists to earn their living as teachers or examiners, while enjoying the opportunity of undertaking personal researches. Thus Nollet, Bossut, Monge, Laplace, Bézout, Arbogast, Lacroix, Callet, Jeaurat, Cousin and Legendre were teachers or examiners in these schools and among the products of the Royal School of Engineering at Mézières alone were Coulomb, de Buat, Tinseau, Meusnier, Carnot, Prieur, Gay-Vernon and Hachette. As well as these military colleges, fully developed at the beginning of the Revolution, it is necessary also to mention *l'Ecole des Ponts et Chaussées* and *l'Ecole des Mines*, where theoretical teaching does not seem to have been at a very high level; as regards the natural sciences, the *Jardin du Roi* (the future *Jardin des Plantes* and *Museum*) had undergone considerable development and, at the beginning of the Revolution, it sheltered as professors or demonstrators a group of naturalists, mostly of great talent: Fourcroy Brongniart, Daubenton, Lacépède, Lamarck, Thouin, Portal, Faujas de Saint-Fond, A.-L. de Jussieu and Bernardin de Saint-Pierre. The routine of university tradition did not allow the faculties of Medicine and Pharmacy to keep up with progress, but there again, the Royal College played a very active role.

To summarise, one can say that at the outbreak of the Revolution the great majority of French scientific men were in favour of important organisational reforms; but, while the ruling classes were in general well disposed towards science, the organisation of French education was notoriously inadequate, especially for elementary education and scientific education. The very structure of the regime and the routinebound attitude of the University were moreover much more responsible for this state of affairs than the personal views of the people in charge, who had often been favourably influenced by the propaganda of the *Philosophes* and the Encyclopaedists. The Revolution was to destroy many of the obstacles which prevented rapid progress and, by a relative democratisation of education and a modernisation of scientific teaching, was to permit the scientific elite to expand in a much broader environment and to take a more active part in the life of the nation.

In 1789 the great majority of members of the Royal Academy of Sciences and of French scientists in general watched with the greatest sympathy the meeting of the *Etats Généraux* and the beginnings of the Constituent Assembly. The part which they had played in the intellectual preparation for the Revolution, their critical sense and their empirical spirit led them to accept enthusiastically all reforms likely to suppress the abuses of the *Ancien Régime*, relieve the misery of the lowest classes of the nation, and realise in concrete terms the ideas of justice and of human liberty, equality and fraternity included in the Declaration of the Rights of Man. This idealist programme was based on the criticisms and proposals contained in the *cahiers de doléances* and was in sympathy with all the plans for reform, conceived and publicised during the previous half century by the Economists, the Philosophers and the Encyclopaedists. But as the new regime evolved, the various scientists took up positions which became clearly differentiated from one another. The problems posed by the fall of the monarchy and the death of the King, by the war against the *émigrés*, against foreign governments and internal enemies, by the more and more revolutionary direction taken by successive governments, by internal struggles in the clubs and the assemblies, by the progressive weakening of liberty of expression, and finally by the Reign of Terror and by the successive reactions which followed it, broke up the fine unity which had appeared in the scientific world.

While the great majority of scientists, carrying out the heavy duties laid upon them by the Assemblies and the various Committees, thus took part

in the great work of national defense and the reorganisation of the country, some of them also mixed in political life, while others tried on the contrary to avoid taking any position which might prejudice them in the eyes of the successive powers. Before outlining the very important role played by the scientists during the revolutionary period we must not pass in silence over various events of which some must be placed on the debit side of the Revolution's balance sheet. Up to 1793, the Academy of Science functioned almost normally and the public powers made frequent use of its services, but when the general political situation hardened, some thought that the continued existence of this body, most of whose members had been elected under the *Ancien Régime* with the consent of the King, and which included honorary members chosen from the nobles of the court, could be considered as a survival of the absolute monarchy. And in order to destroy a possible focus of counter-revolutionary agitation, the Academy of Science was suppressed, along with the other Academies, on the 8th August 1793. This measure, certainly inopportune, was perhaps a result of personal rivalries, but it was necessary to wait till 22nd August, 1795 to see the Academy reconstituted as a section of the National Institute which held its first formal session on the 4th April 1796.

Other purgatory measures, clumsy and often mutually contradictory, also hindered the working of various scientific organisations. Thus while Lagrange found himself at one time threatened with exile, Laplace, Borda, Lavoisier, Coulomb, and Delambre were excluded on the 23rd December 1793 from the Commission of Weights & Measures as having insufficient republican virtue and hatred of royalty to be worthy of confidence. The working of this Commission, created to establish the scientific basis of the metric system, was thus compromised to such a point that it did not really recover until after the amnesty which followed Thermidor. Even more grave actions could be set against the Revolution; the execution, under the Terror, of several scientists, including Lavoisier, the creator of modern chemistry, guillotined at the same time as other tax-farmers, the astronomer Bailly, former Mayor of Paris, de Dietrich, Bochart de Saron and Malesherbes. Several other less important scientists also perished on the scaffold or in prison, while Condorcet committed suicide to escape the guillotine. Certainly the liabilities of the Revolution seem heavy in this respect, but in passing judgment it is necessary to take account of exceptional circumstances—of the climate of denunciation which is inseparable from any period of turbulence and of the fact that if, rightly or

wrongly, these men were condemned, it was entirely for their political attitude, past or present, and not because of any anti-scientific bias among the political leaders.

Moreover, the majority of scientists who retained an almost neutral attitude during the whole Revolution, avoiding any close connection with the successive regimes, came through the revolutionary period without any great difficulty. And far from having been an enemy of science, the Revolution was one of those periods in which, on the whole, the relations between scientists and Government organisations were closest and most fertile. Already the Constituent Assembly contained several members of the Academy of Sciences: Cassini, Condorcet, Lavoisier, Bailly; several patriotic societies and revolutionary clubs also counted eminent scientists among their members; the successive assemblies and many of their commissions remained true to this tradition, and in general the scientists did very useful work in them. But the necessities of national defence imposed on the Governments an almost super-human organisational task. The country was attacked on all sides, and to struggle against external enemies it was necessary not only to raise and train armies, but also to provide them with all the necessary arms and equipment—an infinitely difficult task in a country cut off from foreign connections and deprived by emigration and imprisonment of much of its personnel. The man who took the principal role in this work was Lazare Carnot, Captain in the Engineers, former pupil of Monge at the School at Mézières, already known as author of an important *Essai sur les Machines*, later to become one of the creators of modern geometry. Having already been a member of the Legislative Assembly and the Convention, he was one of the most active members of the famous Committee of Public Safety. In this position, he had to undertake, in very difficult conditions, the task of reorganising the army, restoring the morale of the troops and the whole nation, and galvanising all these for a struggle to the death. In his immense work as "Organiser of Victory" he knew how to find among the best known scientists peculiarly competent helpers, who proved themselves to be valuable organisers. Thus in particular Gaspard Monge, who was already known at this time as a geometer and physicist and had already been Minister of the Navy from August 1792 to May 1793, in collaboration with the mathematician Vandermonde and the chemists Berthollet, D'Arcet, Vauquelin and Hassenfratz, organised the production of arms, munitions and other equipment necessary for the carrying on of the War. This first defence research service had to solve the most complex problems

and was capable of doing so because of the courage, the persistence, and the spirit of initiative, as well as the diversity of talents and technical experience of its members. They had to improvise arms factories, put into practice new methods of foundry work and manufacture, train advisers, draw up detailed instructions for the workmen, produce essential raw materials and in particular find within the country the saltpetre necessary for preparing gunpowder. The impulse given in the scientific world to technical studies since the publication of the *Encyclopédie* was fully repaid in the success of this enormous task. And in collaborating in the most direct way in the national life, the scientists acquired a new prestige; while the interest which they brought to technical questions was reinforced by the success of their work, Government agencies became more clearly conscious of the importance of science, pure and applied, and of the necessity of making science a top-level national concern. In thus rising to meet such difficult circumstances, and in maintaining close contact with the other defenders of the Republic, these scientists definitely destroyed the prejudice which represented them as dwellers in ivory towers, only able to solve theoretical problems in the calm of their retreats. The various regimes which followed the Convention did not forget this and Bonaparte, in particular, always surrounded himself with numerous scientists who had to study and solve technical problems which he set them, while continuing to play their part in the progress of science.

The essential aspect of the effect of the French Revolution that concerns us was the reorganisation of education which it brought about, and which, though less brilliant and less broad than was desired, nevertheless was to bear very valuable fruits. We shall not return to the study of education in France before the Revolution. "Educational establishments", said some of the *cahiers de doléances* from the nobility, "are absolutely lacking in certain parts of the kingdom; those which exist are almost always imperfect; these foundations, mostly very old, have retained the routine of the centuries in which they began". Plans for reform were put forward in 1763 and 1768, and Turgot had called in vain for the creation of a Council of Public Instruction having authority over all educational establishments and even over the Academies.

At the meeting of the *Etats Généraux* in 1789, many *cahiers de doléances* emphasised the need for a reform of the whole system of national education and for the extension of education to all classes of society, under state control. The Constituent Assembly voted a plan on these lines which introduced free education. But the report prepared by Talleyrand was

shelved by the Legislative Assembly. During this time, existing education was disorganised as a result of the breakdown of the financial system and the abolition of the privileges of many establishments, and so the desired reorganisation became more urgent every day. The Committees of Public Instruction elected by the Legislative Assembly and by the Convention attacked the many problems posed by this reform. But the task was enormous, for the whole system of education had to be remoulded or created. Scientific education, in particular, had to be practically created from the beginning, and the manpower for doing it hardly existed. These difficulties explain the delays and the inconsistency of successive decisions.

Of the many plans studied, that of Condorcet was undoubtedly the best, and although it was rejected in principle, it partly inspired all later plans. The object of national education for Condorcet was to establish in reality the political equality between all citizens recognised by the law. A primary school was to be provided for each village, a secondary school for each town of 4,000 inhabitants, an institute for each Department, new institutions of higher education for the country as a whole and, at Paris, a central organisation responsible for co-ordinating education, and all literary, scientific and artistic activities. Amongst other points in the plan, we may mention the creation of numerous libraries and laboratories for physical and natural science. Adopted in principle at first, this plan, certainly too advanced for its time, was later rejected by the Convention.

Effective steps were taken by successive decrees. Although very inferior in conception to the grandiose plans of Condorcet, they had nevertheless the merit of creating a fairly coherent system of education, and one very much superior to that which had previously existed. Primary education was developed on broad lines, and allowed the children of the *petite bourgeoisie*, if not those of the masses, to acquire the essential rudiments of knowledge. Secondary schools (Central Schools), set up in the chief town of each Department, had very varied fortunes before they were transformed under Napoleon into Lyceums or Colleges. The free schools, soon permitted again, provided serious competition for them, at the same time learning from their experience and their programmes, in which for the first time in France, scientific education took an important place.

From the beginning of 1795, courses were organised at Paris to train the future teachers for the Central Schools. This ephemeral "Ecole Normale de l'an III", received 12,000 pupils during three months of winter, in circumstances of great material difficulty. Although the recruitment of pupils was far from uniform, this very brief stage had happy

effects on the cultivation of science in France, for the education given there, though elementary, was of great value. The most famous scientists—Lagrange, Laplace, Monge, Berthollet, Haüy, Daubenton, Vandermonde and others— had actually agreed to become teachers there. The success of this experiment was the origin of the movement which, during the 19th century, moved most eminent scientists to devote part of their efforts to higher education.

At the same time as the *Ecole normale* another educational establishment began whose influence was to be yet greater—the *Ecole Polytechnique*, at first called the Central School of Public Works. This school was intended to train engineers of all kinds, having a profound knowledge of the exact sciences, trained in all the practical work related to their profession and able, if necessary, to work in fields other than those in which they had originally specialised. Monge, who had been in close contact with the reorganisation of teaching at the school at Mézières, made a great contribution to the creation of this new school, to the planning of its syllabuses, and to the practical working out of its teaching methods. Mathematics took an important place in the syllabus, particularly descriptive geometry, which its creator considered to be an essential tool for architects, artists, engineers and workers of all kinds. Seeing in this an instrument capable of playing an effective part in the progress of the country's industry, he wished to see its use extended rapidly. This wish was granted and from courses given by Monge in the Normal School and the Polytechnic began the general diffusion of this new branch of geometry which, in the hands of his pupils, was to show its usefulness in technology as much as in geometry. The courses at the Normal school having been taken down in shorthand and collected into books by order of the Convention, the teaching of the various professors could spread beyond the circle of their pupils. Monge's celebrated treatise on descriptive geometry was only a transcription of these notes, and it is probable that its author would not otherwise have written it. The physical and chemical sciences were taught at the Polytechnic both in theory and by experiment, and the importance thus given to laboratories and to practical work was an innovation of the greatest importance. The techniques themselves were studied there both in courses of lectures and in practical classes, and the pupils had to learn the theoretical foundations of the practical arts as well as their purely practical aspects.

After a difficult beginning amid innumerable obstacles and ever-recurring discussions about the best direction to give to the studies, the

Polytechnic achieved a brillant success which is illustrated by the number of eminent scientists who were educated there, especially before 1840. Napoleon reorganised the School, militarising its internal organisation, and directing the greater part of its pupils into the army. This change, opposed in vain by Monge, certainly prejudiced recruitment to the School and was harmful to its scientific work. It was partly intended to combat the liberal outlook of the pupils and the republican tradition which had continued inside the School. But the School for long remained faithful to its origin, and the Restoration had to struggle against its liberal tradition and, with this object, to recast its statutes several times. Yet the brilliant success of its early years had given the Polytechnic an incomparable prestige which these incidents could not shake. This prestige was such that many pupils of the School were sent or called to various foreign countries, where important technical or educational missions were entrusted to them. Admittedly this is partly explained by the prestige of Revolutionary France itself, and by the extent of the Napoleonic conquests, but the renown of the School, attested by the evidence of many scientists and politicians of all countries, was the essential cause. The quality of the scientific education given at the Polytechnic is also proved, not only by the number and quality of the scientists which it produced, but also by the enthusiasm which it gave to many pupils of modest ability who, wherever they went, became ardent propagandists of the modern scientific culture which had been taught them at the School. While a certain number of foreign students were admitted to these courses, many similar schools were founded in Germany, at Prague, in Italy, and elsewhere, and one can say that the Polytechnic is the prototype of the many technical institutes created during the 19th century.

Two journals, the *Journal de L'Ecole Polytechnique* and the *Correspondance sur l'Ecole Polytechnique*, were published under the patronage of the School to propagate the discoveries of its teachers and pupils. They are of very great interest for the study of the history of science at the beginning of the 19th century, a period in which the Polytechnic was undoubtedly the most fertile scientific centre. From it, in fact, came a great part of the new ideas which contributed to the recasting of science during the first half of the century; the reconstruction of geometry, by Monge and his numerous followers, the introduction of a new rigour in analysis by Lagrange, Lacroix and Cauchy, the development of the experimental method in physics and chemistry, the creation of mathematical physics by Laplace, Fourier and Lamé, the theory of light by Biot, Malus, Arago

and Fresnel, electro-magnetic theory by Ampère, the birth of thermodynamics by Sadi Carnot, the development and perfecting of the new chemistry by Berthollet and Gay-Lussac, and so on.

Its influence was backed up also by that of the first important mathematical journal, the *Annales de mathématiques pures et appliquées* of Gergonne, the introduction of the methods of Leibniz in England by Herschel, Peacock & Babbage, the re-birth of geometrical researches in Germany by Plücker and Möbius and many other events which considerably influenced the evolution of science during the 19th century.

But the influence of the School showed itself also with much effect in all sectors of technology—mechanics, public works, military science, etc. —and also in the creation of various philosophical and political trends which played an important role in the evolution of thought during the 19th century—Saint-Simonism, positivism, Fourierism, etc. In fact, without being really philosophical, the education at the school was permeated by a spirit of science and technical progress, and the political ideals of many former pupils lay in the conception of a basically democratic society in which scientists would play a prominent part in the conduct of affairs of state.

We shall pass more rapidly over the other creations of the Revolution in the scientific field. After their very existence had seemed to be gravely compromised by the success of the Polytechnic, the other great technical schools rapidly took on a new life, adapting themselves to new methods and new educational programmes. Thus *l'Ecole des Mines*, *l'Ecole des Ponts & Chaussées* and *l'Ecole du Génie* were rejuvenated and strengthened. The first *Conservatoire des arts et métiers*, created before the Revolution on the basis of Vaucanson's collections, was considerably enlarged in 1794 and, a few decades later, this institution became also a technical school. The Observatory, which had fallen into a dose by the eve of the Revolution, was also reorganised; the parallel creation of the *Bureau des Longitudes* in 1795 allowed various astronomical and geodetic calculations to be undertaken very systematically and permitted the extension of the annual ephemeris which had been published since 1679 under the title *Connaissance des Temps*. The *Collège de France*. the only school which continued to function normally throughout the Revolution, also saw the scale of its teaching extended, while the *Jardin des Plantes* became a true teaching establishment equipped with laboratories and well adapted to the modern teaching of the natural sciences. The old faculties of medicine, suppressed in 1792, were replaced by schools of health, where theoretical education

was complemented by extensive practical work. Finally, if the Universities were not immediately well adapted to modern scientific education, the creation of indepedent faculties of science by Napoleon in 1808 gave to higher theoretical education an importance which it had never previously had in France and allowed young men who were not intending to become engineers or military officers to acquire a solid scientific foundation.

This creation of a valuable coherent system of scientific and technical education is certainly the most important contribution of the French Revolution to the progress of science, for it allowed a number of men, who under the *ancien régime* would have remained unknown, to show their talents. While falling far short of Condorcet's ambitious plan, this reform did conquer numerous obstacles; its success was due to the prestige which the scientists of the Year III had given to science, the energy and devotion and the organisational qualities of those who undertook the work—particularly Monge, Lakanal, l'Abbé Gregoire and Prieur—and the enthusiasm aroused among the young men by the revival of the national spirit and by the talents of their teachers.

The totalitarianism of the Empire and the political reaction of the Restoration, though they fought their hardest against the thoroughly liberal spirit of these institutions, could neither suppress them nor completely abolish their democratic character. It is for that reason that their dynamic influence, directed both towards scientific progress and towards social reform, can be felt throughout the 19th century, both in France and in various neighbouring countries. It is true that after having served the politics of emancipation in the Revolution, many scientists put themselves, without a second thought, at the service of the Empire; but this was essentially due to the awakening of the nationalistic spirit brought about by the victories of the revolutionary armies, and to the political acumen and cleverness of the Emperor, who always gave science a place on the highest level. The very important works of the *Commission des Sciences* and of the *Institut d'Egypte* and the part taken by scientists in the struggle to overcome the difficulties produced by the continental blockade are in fact in line with the work of the scientists of the Revolution, who, in putting all their energy and talent at the service of liberty, placed themselves at the same time under the most direct state authority, a fact from which later regimes managed to profit.

Another important innovation of the French Revolution was the establishment of the metric system. The great number of units of measure previously used, the complicated relations between them, and their variation

from one place to another, were a constant source of trouble to the scientists, the merchants and the public. The need for a uniform system of weights and measures and for precise standards had been felt even in the 17th century. While beginning to write their experimental results in decimal numbers to avoid some part of the complications of the existing systems of weights and measures, various scientists thought about the possibility of using new units of measure. For the unit of length they thought of the seconds pendulum; and when they had discovered its variation with latitude, they thought of a length connected with the size of the earth. After various unsuccessful attempts, the urgency of a reform became clearly more acute, and many of the *cahiers de doléances* demanded it energetically. And, what the absolute Monarchy had never been able to undertake, the Revolution did by sheer tenacity: As early as 1790, Talleyrand presented a report on the proposed reform, insisting on the necessity to aim at making it international and, for that reason, to choose only natural units likely to be adopted everywhere. The Academy of Sciences was consulted as to what units should be adopted, and drew up the plan for the preparatory work: the construction of various precision instruments, vast surveying operations to give a better measure of an arc of the meridian, detailed works on corrections, auxilliary measures, construction of standards, and so on. After being seriously held back by political and economic difficulties, this immense plan was brought to fruition, and on June 22nd 1799 the standards were presented to the Legislative Assemblies and formally deposited in the Archives. The adoption of the new system encountered numerous obstacles and proceeded rather slowly. It was not definitively achieved till 1840, and since then it has made constant progress in the international sphere.

The decimal division was also extended to angles, and Prony directed the computation of trigonometrical and logarithmic tables in the new units, tables which, unfortunately, were too extensive to be published. The attempt to reform the calendar on something approaching a decimal system was less happy and, after being used for several years, the republican calendar was repealed by the Empire.

Without pretending to have come even near to exhausting the study of the influence of the French Revolution on science, this brief essay nevertheless brings out a series of important relations. Admittedly the fertility of the scientific researches carried out in France at the end of the 18th century and the beginning of the 19th was due in part to the fact that at this time many branches of science were at a turning point in their

evolution, while France possessed an elite of scientists which had been formed under the *ancien régime*. But we cannot deny that the impetus given by the Revolution, the enthusiasm which it aroused, especially among the young, and the much more favourable conditions which it gave to scientific studies and research, were essential factors in bringing about this brilliant scientific revival. And, while the influence of the French Revolution continued all through the 19th century to have effects in the political, social and economic fields, it led in various countries to organisational reforms and scientific works which contributed to define the characteristics of modern science.

## SELECT BIBLIOGRAPHY

Arago: *Notices biographiques*, 3 vols., Paris, 1854.
Aulard: *Recueil des Actes du Comité de Salut public*, 16 vols., Paris, 1889–1925.
Biot: *Essai sur l'histoire générale des Sciences pendant la Révolution*, Paris, an IX–1803.
Ballot: *L'Introduction du machinisme dans l'industrie française*, Paris, 1923.
Bouchard: *Guyton-Morveau chimiste et conventionnel*, Paris, 1938.
— *Un organisateur de la victoire, Prieur de la Côte d'Or*, Paris, 1946.
Caron: *Manuel pratique pour l'étude de la Révolution française*, new ed., Paris, 1947.
Charles-Roux: *Bonaparte gouverneur d'Egypte*, Paris, 1932.
*Centenaire du Museum d'Histoire Naturelle*, volume commémoratif, Paris, 1893.
*Le Collège de France (1530–1930)*, volume jubilaire, Paris, 1932.
*Correspondance de l'Empereur Napoléon Ier.*, 28 vols., Paris, 1857–69.
Cuvier: *Rapport historique sur les progrès des sciences naturelles depuis 1789, et sur leur état actuel*, Paris, 1810.
Delambre: *Rapport historique sur les progrès des sciences mathématiques depuis 1789 et sur leur état actuel*, Paris, 1810.
*Dictionnaire chronologique et raisonné des découvertes en France de 1789 à 1820*, 16 vols., Paris.
Dupin: *Essai historique sur les services et les travaux scientifiques de Gaspard Monge*, Paris, 1819.
Dupuy: *L'Ecole Normale de l'an III* in *Le Centenaire de l'Ecole Normale, 1795–1895*, Paris, 1895.
*Ecole Polytechnique: Livre du Centenaire, 1794–1894*, 3 vols., Paris, 1894–1897.
*Encyclopédie Méthodique: Mathématiques, Dictionnaire de physique, Dictionnaire de Chimie*, 3–5–6 vols., Paris, 1784–1822.
*L' "Encyclopédie" et le progrès des sciences et des techniques*, Paris, 1952.
Fourcy: *Histoire de l'Ecole Polytechnique*, Paris, 1828.
Favre: *Les origines du système métrique*, Paris, 1933.
Guémard: *Essai d'Histoire de l'Institut d'Egypte et de la Commission des Sciences et des Arts* (*Bul. de l'Inst. D'Egypte*, v. 6, 1924, pp. 43–84 & 135–157).
Guillaume: *Procès-verbaux du Comité d'Instruction publique*. 7 vols. Paris, 1889–1907.

Gouhier: *La jeunesse d'Auguste Comte et la formation du positivisme*, 3 vols., Paris, 1934–1942.

Hachette: *Notice sur la création de l'Ecole Polytechnique*, Paris, n. d.

Hoppe: *Histoire de la Physique*, French translation, Paris, 1928.

Jacobi: *Über die Pariser Polytechnische Schule* (*Gesammelte Werke*, v. 7, Berlin, 1891, pp. 355–370).

Jonquière (C. de La): *L'Expédition d'Egypte (1798–1801)*, 5 vols., Paris, 1899–1907.

Klein: *Vorlesungen über die Entwicklung der Mathematik im 19. Jahrhundert*, v. 1, Berlin, 1926.

Lacroix: *Essais sur l'enseignement en général ...*, Paris, 1805.

Landrieu: *Lamarck, le fondateur du transformisme*, Paris, 1909.

Launay (L. de): *Un grand français: Monge, fondateur de l'Ecole Polytechnique*, Paris, 1933.

Lefebvre, Guyot and Sagnac: *La Révolution française*, 3d ed., Paris, 1951.

Lefebvre: *Napoléon*, 3d ed., Paris, 1947.

Lilley: *Social Aspects of the History of Science* (Arch. Int. Hist. Sci., v. 2, 1949, 376–443).

— "*Nicholson's Journal*" (*1797–1813*) (Annals of Science, v. 6, 1948, pp. 78–101).

Maindron: *L'Académie des Sciences*, Paris, 1888.

Martin and Walther: Bibliothèque Nationale, Département des Imprimés. *Catalogue de l'Histoire de la Révolution française* and *Répertoire de l'histoire de la Révolution française*, 6 vols., Paris, 1936–1951.

Mascart: *La vie et les travaux de Jean-Charles de Borda*, Lyon, 1919.

Monzie: (A de): *Le Conservatoire du peuple*, Paris, 1949.

Nielsen: *Géomètres français du XVIIIe siècle*, Copenhagen, 1935.

— *Géomètres français sous la Révolution*, Paris, 1929.

Pouchet: *Les sciences pendant la Terreur*, Paris, 1896.

Reinhard: *Le grand Carnot*, 2 vols., Paris, 1950–1952.

Richard: *Le Comité de Salut public et les fabrications de guerre sous la Terreur*, Paris, 1922.

Sergescu: *Mathématiciens français du temps de la Révolution française*, (Analele Ac. Rom. Memor. Sect. Stiint., v. 16, 1940, pp. 513–560).

Staroselskaïa-Nikitina: *Esquisse de l'histoire des sciences et des techniques pendant la période de la Révolution bourgeoise française 1789–1794*, Moscow-Leningrad, 1946 (title and text in Russian).

Struik: *Outline of a History of Differential Geometry* (Isis, v. 19, 1933, pp. 92–120; v. 20, 1934, pp. 161–191).

— *A concise History of Mathematics*, 2 vols., New York, 1948.

Taton: *L'évolution des méthodes des sciences exactes au cours du XVIIIe siècle et du début du XIXe siècle*, Paris, 1951 (in *Actes du Congrès International dePhilosophie des Sciences, Paris, 1949*).

— *L'œuvre scientifique de Monge*, Paris, 1951.

# THE IDEA OF PROGRESS AND THEORIES OF EVOLUTION IN SCIENCE

by

S. F. Mason*

The theory that biological organisms have evolved from simple to more complex forms, and the idea that man has progressed from rude beginnings to civilised society, appear to have been associated with one another historically. Both views were particular aspects of more general philosophies of development which conceived of man and other living creatures as having originated from the same simple organic forms, and that saw human progress as a continuation of biological evolution, as in the theories of Anaximander and Democritus amongst the pre-Socratic Greeks, or the Epicureans in later Antiquity. Similarly the view that man has reached the limit of his development, has been associated historically with the idea that the organic species are more or less fixed, being created in their present forms by an intelligent First Cause. The two general viewpoints were not necessarily opposed, for Democritus and the Epicureans thought of each world in the universe, and its inhabitants, as evolving, decaying, and then reforming, so that the cosmic process as a whole was an oscillation about a fixed level: there was not an overall evolution.

Elements from both viewpoints in fact crystallised out in later Antiquity to compose an opinion that was generally received by the men of the scholarly tradition down to modern times. Man and nature, it was thought, were much the same throughout the ages, both moving in a cycle about a standard mean: man had reached the limit of his achievement whilst animals and plants had been created in their present forms by an intelligent First Cause. The Roman Emperor, Marcus Aurelius, 121–180 A.D., wrote that the rational soul of man, "Goeth about the whole

* M. A., D. Phil., Research Fellow in Medical Chemistry, The Australian National University, London N. W. 1.

*Centaurus* 1953: 3: 90–106

universe and the void surrounding it, and traces its plan, and stretches forth into the infinitude of time, and comprehends the cyclical regeneration of all things, and takes stock of it, and discerns that our children will see nothing afresh, just as our fathers too never saw anything more than we". More than a thousand years later, the Florentine philosopher, Niccolo Machiavelli, 1469–1527, expressed exactly the same sentiment when he wrote that, "it is ordained by Providence that there should be a continual ebb and flow in the things of this world: as soon as they arrive at their utmost perfection and can ascend no higher, they must of necessity decline; and on the other hand, when they have fallen to the lowest degree, they begin to rise again".

In popular thought the idea that mankind had degenerated was prominent, for the Greek word *presbiteros*, and the Latin word *antiquior*, meant not only "older", but also "better", a conception that was given greater force by the Christian doctrine of the Fall of Man. On the other hand craftsmen and engineers, and scholars interested in the crafts, appear to have had a fairly clear idea of technological progress, and even of progress in scientific knowledge. Aristotle was of the opinion that the development of the mechanical arts had already been completed, but the Alexandrian engineer, Philo of Byzantium, noted that war machines had been improved in his time, "partly by learning from the earlier constructors, partly by observation of later trials". The Roman philosopher, Seneca, who objected to scholars depreciating the craftsmen,—"Posidonius came very near declaring that even the cobbler's trade was the discovery of the philosopher", he wrote,—thought similarly that the sciences would advance, and that in time even the mystery of comets would be explained.

Such views became prominent in early modern times when craftsmen became more widely literate, and a considerable number of scholars interested themselves in the crafts. One of the more notable of such scholars was Francis Bacon, 1561–1626, who pointed to gunpowder, printing, and the magnetic compass, like others before him, as examples of technological progress, since they were unknown in antiquity. But there were two points about these inventions which Bacon was the first to emphasise, and which led him to elaborate his scientific method. The first was that these inventions had been made blindly or accidentally, without foresight or theoretical knowledge, and thus that further advance by this method would be chancey and slow. The second was that these inventions were based upon principles that were entirely different from those

employed in the crafts they had enriched. Printing was not a method of writing more quickly, whilst firearms were dependent upon a principle very different from that used in the bow and arrow.

Bacon suggested therefore that the crafts could be best improved by a conscious search for new scientific principles, for these would yield a rich harvest of new applications. Such principles were to be obtained by induction from experimentally discovered facts and empirical craft knowledge, or in general from observations derived from the active interference with nature's processes, which Bacon considered to be more revealing than the passive contemplation of nature, where the human mind too readily picked out those facts which supported its preconceived notions. The new scientific principles thus discovered would not only lead to new practical applications, but they could also be elaborated by a further process of induction into a new system of natural philosophy. Bacon thought that the crafts could be progressively developed in this way, but he did not think of scientific knowledge as progressing indefinitely. The new natural philosophy was to be a closed and final system of scientific knowledge, like the systems of antiquity it was designed to replace. Given sufficient facts, Bacon thought that he himself could build up a definitive system of natural philosophy. "So far as the work of the intellect is concerned", he wrote, "I may perhaps successfully accomplish it by my own powers, but the materials for the intellect to work upon are so widely scattered that, to borrow a metaphor from the world of commerce, factors and merchants must seek them out from all sides and import them". But once this has been accomplished, "the investigation of nature and of all the science will be the work of a few years".

A little later, Descartes also attempted to formulate a general methodology for the sciences. His views were complementary to those of Bacon, for he emphasised the use of deductive mathematical reasoning in establishing scientific generalities. But, like Bacon, he held that the application of his method would yield a finished system of natural philosophy. Descartes was of the view that he had worked out such a system in his mechanical vortex theory of the universe. He concluded his *Principles of Philosophy*, published in 1644, with the worlds, "No phenomenon of nature has been omitted in the explanations given in this treatise". Descartes also regarded his rules as the final statement of scientific method: he wrote that he did not think, "there is any road by which the human intellect could ever discover better ones". Bacon, who was a little nearer to the craft tradition with its sense of cumulative advance, did not regard his rules as the

definitive method of science. "I merely claim that my rules will make the process quicker and more reliable", Bacon wrote, "I do not mean to say that they cannot be improved upon. This would be utterly at varience with my way of thinking. My habit is to consider the mind, not only in its own faculities, but in close connection with things. It follows that I must admit that the art of discovery itself will advance as discoveries advance". Descartes too only had the idea of progress in connection with practical matters. He said that he wished, "to induce intelligent men to try to advance farther by contributing, each according to his inclination and ability, to the necessary experiments and also by publishing their findings. Thus the last would start where their predecessors had stopped, and by joining the lives and works of many people, we would proceed much farther together than each would have done by himself".

In these ways, the finality of the old scholarly system builders, and the cumulative empiricism of the craft tradition, were expressed together by the philosophers of the early modern scientific movement. Bacon had the idea of progress a little more than Descartes, being nearer to the craft tradition, the empiricism of which he reflected in his method, and also in the fact that he did not work out the new natural philosophy he had announced. Descartes, with his orientation towards the scholarly tradition, did elaborate a new system of natural philosophy, which in its content as well as its form expressed some of the values of the ancient philosophers. At first sight Descartes' universe appears to be a world in evolution: the giant cosmic vortices had fashioned the primordial matter of the universe, according to the laws of mechanics, until it had assumed the arrangements we observe today. But Descartes was concerned to stress that any possible world of primordial matter would necessarily assume the present configuration of our world and become stabilised in that form, as the laws of mechanics would always operate in the same way. Thus our present world was the predetermined end of any cosmic system: it was in fact the only possible world. Malebranche, Descartes' disciple, thought that the universe was an excellent piece of workmanship considering that it had been formed with only a few general principles, namely the laws of mechanics. Leibniz went further and insisted that our world is the best of all possible worlds, no improvement whatsoever could be envisaged.

When English philosophy spread to the continent during the 18th century, it was widely thought that Newton had finally constructed the new and definitive system of the world that had been advertised by Bacon and Descartes. Voltaire, who was perhaps the most important figure in

the transmission of English thought to France, affirmed in a letter to Horace Walpole that, "Newton pushed his work to the most daring truths which the human mind could ever reach". Later in the century Lagrange wrote of Newton that, "There is but one universe, and it can happen to but one man in the world's history to be the interpreter of its laws". Newton had thought of the universe as a product of evolutionary development even less than Descartes. For Newton, God had created the world in the form in which it is found today, and only then had the laws of mechanics come into operation to sustain the cosmic machine. There were phenomena, Newton thought, which were not entirely explicable in terms of the laws of mechanics, but Laplace towards the end of the 18th century tied up these loose ends, and showed the solar system to be mechanically stable.

In this mechanical world of the 18th century nothing had developed historically, all the inhabitants and the creatures of the earth had existed in their present forms from the beginning. Animals and plants were machines, but they were undoubtedly complex machines and could not have generated themselves spontaneously from matter and motion. Like the world as a whole, they were constructed in their present forms at the beginning of time, and so were all future generations of creatures. A London physician, Cheyne, remarked in 1715, "If animals and vegetables cannot be produced from matter and motion, (and I have clearly proved that they cannot), they must of necessity have existed from all eternity". Thus it was widely thought that the first animals and plants had contained within themselves facsimile minatures, like a series of boxes one inside the other, which constituted all the future generations of the species, each new born creature being an enlargement of a preformed small scale model.

Thus the formation of the world and all its inhabitants, like the place of the scientific revolution in history, was seen during the 17th and 18th centuries as a single creative event, which once accomplished, was eternally enduring and finished for all time. During the same period a similar view obtained concerning the formation of human society. For all their differences, Bodin, Hobbes, Locke, and Rousseau, thought that once upon a time isolated individual men had come together and had contracted to live with one another in human society for ever after. The mechanical ideal was extended to human society in other ways by Bodin in 1577 and Montesquieu in 1748. They held that the geographical location and the climate of a region in which a nation lived determined its national character. The men of the north tend to be vigorous but not

very intelligent, they thought, whilst proceeding southwards, men become cleverer but more feeble. Mankind could not avoid these things, they were external determinations laid down by the environment. Others, such as Hume, held that social institutions determined national character, but they agreed that mankind had been much the same throughout the ages after the signing of the original social contract. Hume averred in 1748 that, "mankind are so much the same in all times and places that history informs us of nothing new in this particular. Its chief use is only to discover the constant and universal principles of human nature".

But it was the extension of the mechanistic viewpoint to the human sphere, to psychology and sociology, that led to the idea of progress, or rather to two somewhat contradictory ideas of progress. At the beginning of the 18th century Fontenelle suggested that if humanity has been much the same at all times and places, then mankind must progress by the sheer accumulation of knowledge throughout the ages. We have no reason to suppose that trees were larger in antiquity than today, he said, and such must be true of nature as a whole and mankind too. These things being so, we must have men of the stature of Homer and Plato amongst us today, and such men can proceed further because they can start where their predecessors left off. Mankind therefore has become progressively enlightened, he held, for at least, "We are under obligation to the ancients for having exhausted all the false theories that could be formed". Voltaire, after Fontenelle, called for an active effort to advance mankind through the criticism of traditional beliefs and the dissemination of the newly acquired knowledge of the natural world. Most the the French philosophers of the 18th century joined in this movement for the progress of mankind through the spread of enlightened opinion, contributing articles to the most important publication of the movement, the great *Encyclopédie*, which came out in thirty three volumes between 1751 and 1777. In the prospectus, issued in 1750, the editor, Diderot, stated that the aims of the work were, "to bring together all the knowledge scattered over the surface of the earth, and thus to build up a general system of thought, so that the works of past ages shall not be useless, and our descendants, becoming more instructed, shall become more virtuous and happier". The *Encyclopédie* was immensely effective, the *Avocat Général*, Seguier, confessing in 1770 that, "the philosophers have shaken the throne and upset the altars through changing public opinion". Such a confession, the philosophers thought, illustrated their contention that "Opinion governs the world".

But they also deduced the opposite view that the world governs opinion, or more specifically, that legal and educational institutions determine opinion, from the mechanical psychological theories of the period. Locke had affirmed that the mind of man at birth was like a blank sheet of paper upon which sensations from the external world wrote all the manifold variety of human experience. It seemed then that men were made what they were by the sum total of the impressions that they had received from their earliest years, human opinion being formed by external forces, notably education and laws. On this view progress was possible through the reform of legislative and educational institutions: indeed some went so far as to declare that France would become a nation of Newtons and Shakespeares if only the appropriate reforms were carried through. A conflict remained however between the views that opinion governs the world, and that the world governs opinion, and also between the two ideas of progress deriving therefrom. The French philosophers of the 18th century did not resolve this dilemma, but the idea of progress, and the dilemma itself, stimulated some of their biologist colleagues to formulate theories of organic evolution.

In biology there was a conception, dating back to Aristotle, that the various organic species form a continuous and linear chain of creatures stretching from minerals at one end, through the various grades of plants and animals, up to man at the other. Such a chain was conceived of as a timeless scale of beings that was full and complete, the highest organism of one class being directly contingent upon the lowest organism of the order above. In the 18th century the idea of progress was merged in with this conception so that the chain of beings was seen not as a static hierarchy of creatures, but as an evolutionary series of organisms in time. The first notable expression of such a view came from Jean Baptiste Robinet, 1735–1820, who published a large work in five volumes *On Nature* between 1761 and 1768. Robinet regarded the organic species as forming a linear and continuous scale of creatures. The Creator, he wrote, "has made all vegetable species which could exist: all minute gradations of animality are filled with as many beings as they can contain". But the chain was not a static vertical scale: all creatures, he affirmed, receive, "additions which they are able to give themselves by virtue of an internal energy, or to receive from the action of external objects upon them". This internal self-differentiating energy he thought, "the most essential and the most universal attribute of being ... a tendency to change for the better". Such an immanent force was a spiritual fire, it was a biological expression

of the same force which on the human plane showed itself in the progress of enlightened opinion. "The human mind must be subject to general law", Robinet wrote, "We cannot see what could arrest the progress of its knowledge, or oppose its development, or stifle the activity of this spirit, all of fire that it is". In the same way the addition that organisms received from external objects was analogous to the psychological conditioning of man by his environment.

The first of the important modern evolutionists, Jean Baptiste Lamarck, 1744–1829, adopted a similar theory in his *Zoological Philosophy*, published in 1809. Like Robinet, Lamarck thought that within each creature there was an inner force that operated continuously for the improvement of the species. If this force were not impeded in any way, it would lead to a perfectly linear series of creatures from simple unicellular organisms up to man. Lamarck saw that the scale of creatures in nature was not at all perfect, and he brought in his doctrine of the inheritance of acquired characteristics to explain this imperfection. In the organic world, he wrote, "Progress in complexity of organisation exhibits anomalies here and there in the general series of animals, due to the influence of the environment". Thus Lamarck broke down his evolutionary line of creatures into a branching tree of animal descent, though his series were more linear than the genealogical trees of later evolutionary theories.

In the sphere of biology Lamarck attempted to resolve the dilemma of the French philosophers of the 18th century as to the relationship between the human mind and its physical environment. "The influence of the physical on the moral has already been recognised" by the psychologists, he wrote, "but it seems to me that sufficent attention has not yet been given to the influence of the moral on the physical". The dilemma arose, Lamarck thought, because the psychologists and biologists had studied man in isolation from other creatures, instead of considering man as the end product of an evolutionary series of animals. "After the organisation of man had been so well studied, as was the case", he wrote, "it was a mistake to examine that organisation for the purposes of an enquity into the causes of life, of physical and moral sensitiveness ... An examination should have been made of the progression which is disclosed in the complexity of organisation from the simplest animal up to man, where it is most complex and perfect. The progression should have been noted in the successive acquisition of the different special organs, and consequently of as many new functions as of organs obtained ... I may add that if this method had been followed ... it would never have been said

that life is a consequence of movements executed by virtue of sensations received by various organs or otherwise: nor that all vital movements are brought about by impressions received by sensitive parts".

The view that the actions of animals and men were governed by impressions and stimuli received from outside had arisen from the idea that they were machines. Such an idea was justified, Lamarck held, if only account were taken of the driving force of the machinery. "A living body may be compared to a watch": he wrote; "As to the machinery of movement, its existence and faculties are well known ... but as to the spring, the essential motive power and originator of all movements and activities, it has hitherto escaped the researches of observers". The inner power of animals that sustained them and provided the driving force of evolution were heat and electricity, Lamarck thought: heat is, "the material soul of living bodies", whilst in the nerves, "the electric fluid ... provides the cause of organic movements and activities". Thus the psychologists were right in supposing that man was conditioned by his environment, and so too were the theorists who supposed that man extended his control of the world through the progress of the mind. "If nature had confined herself to her original method", Lamarck wrote, "that is, to a force purely external and foreign to the animal, her work would have remained very imperfect: animals would have been simply passive machines, and nature would never have produced in such organisms the wonderful phenomena of sensibility, the intimate feeling of existence, the power of acting, and lastly, ideas, by means of which she created the most astonishing of all, viz. thought, or intelligence".

In these ways Robinet, and more particularly Lamarck, carried over into biology the preoccupations of their age, namely the idea of progress, and the problem of the relation between the mind of man and his physical environment, both of which had been developed from the mechanical philosophy through the stimulus of the French political and intellectual movements of the period. Lamarck's theory of evolution was not widely accepted in his time, for it was associated with the views of the French materialists of the 18th century, whose opinions became unfashionable in official circles in France, particularly after the restoration of the Bourbons in 1815. The ideas of Lamarck were strongly opposed by the biologist, Cuvier, who stood high in French official circles. In 1830 Cuvier brought the matter up before the Paris Academy of Sciences, and after a historic debate, he succeeded in extinguishing the idea of organic evolution in France, until Darwinism made its impact late in the 19th century.

In England meanwhile events had taken rather a different course. Bacon's project for the progress of industry through the advance and application of science had been adopted by the Royal Society in the first years after its foundation in 1662, but little was accomplished, and enthusiasm for the project waned. In the opening year of the 18th century, the Council of the Royal Society regretfully placed it on record that, "the discouraging neglect of the great, the impetuous contradiction of the ignorant, and the reproaches of the unreasonable, had unhappily thwarted them in their design to perpetuate a series of useful inventions". It was then that the conception of Leibniz that this is the best of all possible worlds became popular in England, as enshrined in Pope's dictum that, "Whatever is, is right". The idea of technological progress had largely evaporated, but the idea of progress in general did not disappear, it was spiritualised. In 1711 Addison suggested in *The Spectator*, that whilst man can do little on earth but reproduce his kind, after death his soul will progress indefinitely towards the highest perfection. "There is not in my opinion", he wrote, "a more pleasing and triumphant consideration in religion than this of the perpetual progress which the soul makes towards the perfection of its nature without ever arriving at a period in it".

Such a conception was popular in England at the time. The psychologist, David Hartley, gave up the study of theology at Cambridge and turned to medicine because he could not accept the doctrine that sinners were eternally damned: they must at some point, he thought, partake of the universal progress of all souls. In his *Observations on Man*, published in 1749, Hartley worked out a psychological mechanism for the temporary punishment of sinners after death. Habits and experience in general excit vibrations in the brain, he thought, which make an impress upon the soul. These impressions persist after death, so that souls, "receive according to the deeds done in the gross body, and reap as is sowed". Eventually the impressions die away, so that, "It is probable from reason that all mankind will be made happy ultimately".

As the industrial revolution gathered momentum in England towards the end of the 18th century the idea of the material progress of mankind revived again, and was particularly marked amongst the members of the Birmingham Lunar Society, which was, so to speak, the scientific general staff of the 18th century industrial revolution in the midlands. Hartley had a considerable influence upon the members of this Society, and two of them, Joseph Priestley and Erasmus Darwin, secularised his theological idea of progress, Darwin applying it to biology, and Priestley to the

material progress of man. Erasmus Darwin in his *Zoonomia*, published in 1794, wrote that, "The ingenious Dr. Hartley in his work on man, and some other philosophers, have been of the opinion that our immortal parts acquires during this life certain habits of action or of sentiment, which become forever indissoluble, continuing after death in a future state of existence ... I would apply this ingenious idea to the generation or production of the embryon, or new animal which partakes so much of the form and propensities of the parent". Thus the habits acquired by an animal during its lifetime are inherited by its offspring, and lead to an evolution of the species. Lamarck a few years later was to put forward a similar but fuller theory of the inheritance of acquired characteristics. Like Lamarck also, Erasmus Darwin thought that there was an inner force within each organism driving it forwards towards higher forms. Each animal possesses, he wrote, "the faculty of continuing to improve by its own inherent activity, and of delivering down these improvements by generation to its posterity world without end".

Erasmus Darwin however had one conception that Lamarck lacked, namely the idea that organisms evolved through the mechanism of competition. Like his grandson, Charles, Erasmus Darwin believed that cocks had developed spurs, and stags grown their antlers, because they had competed with one another for the females of their species. Similarly many changes in plants, he said, "seem to have arisen in them by their perpetual contest for light and air above ground, and for food and moisture beneath the soil". Such a conception, with its flavour of laissez faire, proved to be both popular and fruitful in Victorian England later when it was elaborated by Charles Darwin.

In 1798, a few years after the work of the elder Darwin, the economist, Robert Malthus, brought out his *Essay on Population* in which he used the mechanism of competition to demonstrate that the progress of mankind was not possible. "I think I may fairly make two postulata", Malthus wrote, "First, that food is necessary to the existence of man. Secondly, that the passion between the sexes is necessary and will remain nearly in its present state". These things being so, he continued, "I say that the power of the population is indefinitely greater than the power of the earth to produce subsistence for man ... (for) population when unchecked increases in geometrical ratio, subsistence only increases in an arithmetic ratio. A slight acquaintance with numbers will show the immensity of the first power in comparison with the second ... Consequently if the premises are just, the argument is conclusive against the

perfectibility of man". Malthus thought that the life of mankind was of a piece with that of the organic world as a whole. "Throughout the animal and vegetable kingdoms", he wrote, "nature has scattered the seeds of life abroad with the most profuse and liberal hand. She has been comparatively sparing in the room and nourishment necessary to rear them. The race of plants and the race of animals shrink under this great restrictive law. And the race of man cannot by any effort of reason escape from it".

It was this conception that provided Charles Darwin with his mechanism of evolution: organisms compete for restricted food supplies, and thus those with favourable variations survive and reproduce their kind. But Darwin inverted Malthus' general conclusion. Competition for food was not a conservative influence, it led to the evolution of higher organisms more adapted to their environment. However Darwin reached the conviction that organic evolution had occurred, some time before he conceived this mechanism to explain it. Darwin started his scientific career as more of a geologist than a naturalist, and it was the theory of the development of the rock strata that led him to suppose that the organic species had similarly evolved. The first important theory of geological evolution appeared in a paper read to the Royal Society of Edinburgh by one of its members, James Hutton, in 1785. Like the French philosophers who derived their idea of the progress of mankind from the view that man has been much the same throughout the ages, Hutton came to the view that there had been an orderly succession of the rock strata in time from the proposition that the geological forces at work throughout the history of the earth had been constant and always the same. The winds, rains, rivers, operating continuously, had laid down stratum after stratum, whilst constant volcanic eruptions had tilted and transformed them. Hutton's theory was not widely accepted at first, for much more work had to be done in geology, and a new generation less resistant to the idea of progress and evolution had to arise before it gained much support. The views of Hutton were adopted and amplified by Charles Lyell, and after the publication of his *Principles of Geology* in 1830 the theory of geological evolution was gradually accepted. Lyell's standpoint was essentially the same as that of Hutton. The geological forces of nature had been constant throughout the history of the earth, and had therefore produced a succession of rock strata in time: such a view was termed uniformitarianism. Lyell at first was so much a uniformitarian that he rejected Lamarck's theory of organic evolution on the grounds that the organic species must have been always the same. However it was pointed

out that each rock stratum contains its own characteristic fossils, and thus if there had been a succession of rock strata in time, so too there must have been a succession of the organic species. In this way Lyell prepared the way for Darwinism.

When a young man, Charles Darwin went upon a voyage of exploration round the world between 1831 and 1836, in the course of which he gathered together a great deal of geological and biological information. He took the first volume of Lyell's *Principles of Geology* with him on the voyage and was soon converted to Lyell's views. Writing home he said, "I am become a zealous disciple of Mr. Lyell's views, as known in his book. Geologising in South America, I am tempted to carry parts to a greater extent even than he does". By the end of the voyage he had become convinced that the animal and plant species were not fixed, but had evolved one from the other by some natural means. He had then no idea as to how this could have happened, but he began to make notes upon the subject. Later he recorded in his autobiography how the leading conception of natural selection came to him. "In October 1838", he wrote, "I happened to read for my amusement 'Malthus on Population', and being well prepared to appreciate the struggle for existence which everywhere goes on from long continued observation of the habits of animals and plants, it at once struck me that under these circumstances favourable variations would tend to be preserved, and unfavourable ones to be destroyed. Here then I had at last got a theory by which to work".

Darwin spent the next twenty years collecting information to substantiate this theory and working out its implications. Meanwhile another English biologist, Alfred Russel Wallace, was working along the same lines and in 1858 independantly discovered the theory of natural selection. Malthus too was his starting point. He recorded in his autobiography that, "In February 1858 ... the problem (of evolution) presented itself to me, and something led me to think of the positive checks described by Malthus in his *Essay on Population*, a work I had read several years before and which had made a deep and permanent impression on my mind. These checks ... must, it occurred to me, act on animals as well as man ... and while pondering vaguely on this fact, there suddenly flashed upon me the idea of the survival of the fittest ... I sketched the draft of my paper ... and sent it by the next post to Mr. Darwin'. Darwin had Wallace's paper published together with one of his own, and in the next year, 1859, he brought out his great work on the *Origin of the Species*. It is perhaps not surprising that both of the men who discovered the theory of organic

evolution by natural selection should have taken the views of Malthus on population as their starting point, for both of them belonged to the generation that was the most strongly influenced by Malthus, and by the British schools of political economy, utilitarianism, and philosophical radicalism more generally. It was the generation moreover that was deeply imbued with the solid and substantial Victorian idea of man's material progress. Herbert Spencer, before the appearance of the *Origin of the Species*, had already inverted the pessimistic conclusions of Malthus. In his *Theory of Population deduced from the General Law of Animal Fertility*, published in 1852, Spencer wrote that, "From the beginning, pressure of population has been the proximate cause of progress. All mankind in turn subject themselves more or less to the disciple described: they either may or may not advance under it, but in the nature of things only those who do advance under it eventually survive". The Utilitarians also seem to have influenced the English theorists of organic evolution, for the organs of animals were judged from the point of view of their utility. Darwin remarked that with certain few exceptions, "the structure of every living creatures either now is, or was formerly, of some direct or indirect use to its possessor'. Bentham's principle that the quest for happiness was the prime motive force in mankind was adopted by Darwin and extended to the organic world: the drive for pleasure, he thought, was the dominant source of action in animals as well as man.

The idea of progress and of evolution had now undergone an important change. Darwin and others regarded organic evolution and human progress as exemplifications of an automatic law of cosmic development that operated independently of the desire and will of man or animals, whilst earlier evolutionists had regarded inner wants and strivings of animals and men as at least one of the factors leading to their advance. In the closing paragraph of the *Descent of Man*, published in 1871, Darwin wrote, "Man may be excused for feeling some pride at having risen, though not through his own exertions, to the very summit of the organic scale; and the fact of his having risen, instead of being placed there aboriginally, may give him hope for a still higher destiny in the distant future". In the same way, Herbert Spencer, the first Social Darwinist, supposed that laissez faire was an automatic law of human progress, the unfettered operation of economic competition mediating the survival of the ablest and most efficent entrepreneur. It was Spencer who coined the phrase, "the survival of the fittest", to describe what he took to be the essence of both biological evolution and human progress.

Spencer was very much a mid-Victorian. Developments in the late Victorian period, the strife of nations as exemplified in the Boer war, filled him with distaste, for it was the pacific and industrious competition of individual men that seemed to him to be the main agent of human progress. However the new developments could be reconciled with the conceptions of Darwinism, indeed they were to some degree anticipated by the historian and economist, Walter Bagehot, who in 1872 published a volume of essays entitled *Physics and Politics, or Thoughts on the Application of the Principles of Natural Selection and Inheritance to Political Society*. In this work Bagehot suggested that, "The strongest nation has always been conquering the weaker", and it is by these means that, "the best qualities wanted in elementary civilisation are propagated and preserved", for, he thought, "the most warlike qualities tend principally to the good". In 1900 Karl Pearson at University College, London, published an essay *On national Life from the Standpoint of Science* in which he expressed similar views. Such interpretations of Darwinism were popular at the period, and are not entirely dead today.

The biologists themselves, generally speaking, were not given to such interpretations. Darwin in his *Descent of Man* saw in the progress and evolution of mankind the growing dominance of the cooperative over the selfish instincts. "The more enduring social instincts conquer the less persistent instincts", he affirmed. Darwin's disciple, Huxley, was much opposed to the conclusions of the Social Darwinists. In an address delivered in 1888 he held that the "Hobbesian war of each against all", obtained only amongst the precursors of man. The formation of human society in itself implied the growth of a force opposed to the primitive struggle for existence. Alfred Russel Wallace, who had arrived at the theory of natural selection independantly of Darwin, deduced the doctrines of the Christian Socialists from the theory in his *Studies Scientific and Social* published in 1900. In the social struggle for existence, he held, none should have an unfair advantage of wealth or education, we must all start equally to get the full progress of mankind. "The only mode of natural selection that can act alike on physical, mental, and moral qualities", he wrote, "will come into play under a social system which gives equal opportunities of culture, training, leisure, and happiness to every individual. This extension of the principle of natural selection as it acts in the animal world generally is, I believe, quite new, and is by far the most important of the new ideas I have given to the world".

Thus in the end almost any theory of human progress could be deduced

from Darwinism, though the more influential interpretations were those that emphasised the competitive element in human society. Towards the end of the 19th century the easy optimism of the mid-Victorian period began to evaporate, and a mood of doubt concerning the progress of the human race set in. In biology at the same time stress was laid upon the stability and the continuity of the organic species rather than upon their mutability and change, notably by August Weismann in Germany, who from the 1880's developed his theory of, "the continuity of the germ plasm". Such a reorientation in biology focussed attention upon the problem of heredity, and led to the development of genetics following the rediscovery of Mendel's work in 1900. In social theory the reorientation gave an impulse to the development of racial doctrines, particularly in Germany where the change originated and was most marked.

Patrick Geddes in his little book on *Evolution*, which he wrote with Arthur Thomson, observed that each of the main theories of biological evolution seemed to be part of the general "social transformations of its age". "The generation of culminating political revolution in France", he wrote, "that of the culmination of the industrial revolution in England, have thus expressed themselves through Lamarck and Darwin more clearly than either thinkers ever dreamed, or than their respective exponents and disciples have realised ... What are Lamarck's interpretations of the effects of use and disuse, his assured insistence upon the interior freedom of the organism to realise its inmost capacities, but the new step in social progress through abandonment of outworn orders of society, the freedom opening before new ones. "*La carrière ouverte aux talents*" is pure Lamarckism; so again the splendid overassurance of the Napoleonic epic, that "every French soldier carried a marshal's baton in his knapsack". But the colder business view so characteristic of English thought came to prevail over such political and military exaggerations; the ideals of mechanical efficiency and of individual and financial success rising about the ruins of liberal aspirations and of imperial achievements as they have so often done ... "Competition is the life of Trade": then why not also the trade of Life? Yet with all this freshness and vigour of economic application, there has prevailed in the main, and still prevails, a naive forgetfulness of the social origins of these naturalists' discoveries. Similarly in neo-Darwinian times. With united and real respect for Weismann, for whose work one of us has once and again acted as translator and editor, the other yet ventures to urge one of the very few criticisms which that wide and fair-minded and subtle thinker seems never to have

considered: the striking parallelism of his own theory of the germ-plasm, with the thought of contemporary Germany: with the victories and hegemony of Prussia, the renewed claims of its aristocracy also; and above all, with its doctrines of race, political and anthropological combined". Geddes' observations have much historical acuity, but they cannot be taken in themselves as criticisms of the theories he analyses. It is a measure of the greatness of Lamarck that he used the ideas of the psychologists and philosophers of his time to some purpose: he filled the formal analogies between man and the animal world with a real empirical content. It is a measure of the genius of Darwin that he explained a much wider body of facts using the ideas of Malthus, the political economists, utilitarians, and philosophical radicals, and in so doing produced a theory of permanent value.

## BIBLIOGRAPHY

J. B. Bury, *The Idea of Progress, an Inquiry into its Origins and Growth.* 1920.

T. Cowles, *Malthus, Darwin, and Bagehot: a Study in the Transference of a Concept.* Isis, 1936, 26, 341.

*The Life of Charles Darwin,* Edited by Francis Darwin, 1892.

Patrick Geddes and Arthur Thomsen, *Evolution,* no date.

E. Krause, *Erasmus Darwin,* 1879.

A. Lovejoy, *The Great Chain of Beings,* 1936.

J. Rosenthal, *The Attitude of some Modern Rationalists to History,* The Journal of the History of Ideas, Volume 4, 1943.

E. Zilsel, *The Genesis of the Concept of Scientific Progress,* The Journal of the History of Ideas, Volume 6, 1945.

# THE INTERPLAY OF SOCIAL AND INTERNAL FACTORS IN MODERN MEDICINE

*An Historical Analysis*

by

Richard H. Shryock*

The simplest classification of the factors involved in the history of a science is that which relates these, first, to the surrounding cultural environment and, second, to the "internal logic" of the science itself. This distinction is not an absolute one[1] but is useful for most purposes, provided that the two categories are broken down into meaningful components. "Cultural environment", for example, includes the technologic, social, and philosophic backgrounds. It also involves professional and institutional circumstances, and independent developments in other sciences. Pertaining to internal logic, in contrast, are the assumptions and objectives of scientists, their approaches (questions asked of Nature), methods employed (logical, and technical), foci of interest, necessary sequences in discovery, evolving ideas, and so on. Also internal to the science is the rôle of individual genius, in so far as this is deemed a distinct influence in a given field.

So defined, a great part of the first category (environmental) relates to the "social history" of science; and a major portion of the second (internal) category to "the history of scientific thought". But one cannot entirely equate these concepts; for example, the rôle of a particular, applied technique is an internal one and yet may have little or no influence on scientific thought.

It was usual, during the nineteenth century, to write the history of science largely in terms of the internal aspects. Although some attention was accorded to professional circumstances and to philosophic backgrounds, little heed was given to the intricacies of the total social *milieu*.

* Wm. H. Welch Professor of the History of Medicine, Director of the Institute of the History of Medicine, Johns Hopkins University, Baltimore, Md., U.S.A.

*Centaurus* 1953: 3: 107–125

This oversight or indifference was carried so far, at least in the case of the medical sciences, as almost to imply that these developed within a social vacuum. In order to correct this tendency, much attention has been devoted during recent decades to the "social relations" or the "social history" of science[2]; but this effort has in turn been carried to extremes. One would gather, from some current works, that the development of the sciences was little more than a function of the general cultural environment[3].

It is not the present purpose to linger with either of these extreme interpretations, although each has its significance for historiography. The view here taken is, rather, that the history of science can be understood only in terms of a constant interplay between internal logic and environment. The omission or even the relative neglect of either of these—however helpful for immediate analysis—will distort any final picture. One may hold, no doubt, that the internal story of a science is of primary concern, in that this is the most distinctive aspect of its history. It is this which makes biology, biology, and not just an indistinguishable thread in a larger pattern. Yet even this statement may be misleading, if it involves emphases not fully justified by the evidence. Much attention, certainly, should be given to the more potent influences to which a science is subjected; and these are not necessarily the most distinctive ones.

The analysis of modern medical history which follows is intended to present—to bring out, as it were—the interplay in that particular field between environmental and internal developments. The sequence employed is a topical rather than a chronologic one, and an attempt is made to be reasonably comprehensive within the limits set by necessary brevity.

One may recall, to begin with, the influence exerted by the European cultural environment as a whole. During the early modern era, pervasive cultural changes—to which science was continuously related as both cause and effect—stimulated and transformed various aspects of "natural philosophy". Medicine[4] then shared with other sciences (as now recognized) the benefits which resulted from certain trends in European life—from trade expansion, the rise of the middle class, the revival of Greek science, new intellectual outlooks, and so on. One need not here review all aspects of the sociology of knowledge, or the varied implications of new perspectives, in order to recall the advantages which society gradually extended to science during this epoch. Certain it is that, by 1750, scientific activities were carried on within a social and intellectual setting which was

far more conducive to discovery than had been that of the Middle Ages. And it need hardly be added that scientists, taking advantage of this, had by 1750 achieved remarkable results in both basic and applied investigations.

Yet the rate of advance varied widely in different fields. If one accepts the use of quantitative methods as a measure of progress, for example, it is clear that dynamics had reached a level by 1650 which chemistry did not attain until after 1775, and which clinical medicine did not even approach until after 1825. This means that, at any given time between these dates, different sciences were operating on different levels of method and of achievement, despite the fact that all were immersed in a common cultural environment. *A priori*, such contrasts could be ascribed to one or both of the following circumstances: (1) differences in the relationships between the common environment and one science, as compared to another, and (2) differences in the respective natures or internal logic of the various sciences as such.

In the case of medicine, the first theme noted above was of unusual significance. It will be recalled that, despite conditions relatively favorable for science by 1750, there was—from the modern viewpoint—still much to be desired in this connection. It is true that universities were maintained with the help of church, state, and private endowments, and that professors in certain of these institutions were encouraged to pursue original work. But there was little or no direct, financial aid for research in itself. The very establishment of scientific academies, which provided moral support, limited publication facilities, and a few terminal awards, was itself evidence of the inadequacies of the older institutions so far as science was concerned. Few outstanding scientists taught in the universities, and the recruiting of future investigators was anything but systematic.

No doubt the rather casual manner in which research was then supported was more effective than it could be today. The prevailing arrangements were fairly well adapted to the needs of the contemporary physical sciences, since most of the latter were still in a relatively simple stage of development. Research procedures were not complex, technical facilities were neither rich nor rare, and little specialized training was required of investigators. Under these circumstances professors could manage reasonably well without "outside aid", and self-trained amateurs could and did do outstanding work.

The adjustment of medicine to the cultural environment[5], however, was less satisfactory. Maladjustment here resulted, in part, from a lack of

*rapport* between medical science and the society of which it was a part. Unlike the physical or the general biologic sciences, medicine dealt directly with the most vital interests of mankind—with birth and death—and out of this situation arose a whole series of peculiar difficulties. Most obvious, first, was the manner in which the human body (as basic subject matter) was hedged about by all sorts of moral taboos. Physical scientists could do as they pleased with test tubes and pendulums, but physicians must not experiment with living men except within very narrow limits. Popular opposition to the dissection of dead bodies lingered into the nineteenth century, and some abhorence of autopsies and even of animal experimentation persists to this day.

Physicians could, of course, learn much about disease by a passive observation of the sick and by some cautious experiments in treatment. But sound generalizations must be based on many cases, and the traditional "solo" form of medical practice did not enable a physician to see more than a small number of patients. What was needed was an institution in which large numbers of cases could be studied; that is, the hospital. But hospitals had been founded chiefly for humane rather than for scientific ends, and it was not until the nineteenth century that many of them were so organized as to be available for systematic investigations.

If a medical man surmounted the obstacles noted, moreover, he faced still another difficulty inherent in what Roger Bacon once called "the nobility" of his materials. This was the fact that physicians were under constant pressure to get results quickly. This was not usually the case with physical scientists, because the latter's findings were rarely of vital concern to the public. Hence physicists, even though seeking "useful knowledge", could suspend judgment and proceed with due caution. But death would not wait, and so men desired that physicians reach conclusions without benefit of real verification. The insistent need for curing illness had been present throughout the centuries, when the state of medical knowledge was such—we can now see—that it could not possibly meet this demand. Yet the attempt had to be made and, what is more, men ever wished to believe that it had at last been successful. Such wishful thinking constantly encouraged guess-work, unverified speculation, or sheer dogmatism in medical thought.

In addition to the difficulties imposed on medical science by moral or other human considerations, there were further obstacles inherent in the European professional tradition. In the case of physical science, there was no large and ancient guild whose organization or vested interests might

retard an effective pursuit of new science. It was far otherwise in medicine. Consider, for example, the diverse manner in which the lack of financial aid for research affected the two fields in the United States. In medicine, by guild tradition in this country, there were rarely any "full-time" professors in medical faculties before 1890. Within the universities, professors of physical science could give all their time to teaching and investigation; but medical instructors were selected from among the best known—and therefore the busiest—practitioners. Such men could pursue original work in their spare moments, as could any self-patron; but the truth was that they had very few moments to spare when lives might be at stake. Cynics may add that professional income was also at stake, but this was not the whole story. Even the wealthy practitioner who was unmoved by humane considerations, and who need never worry about "that damned guinea", found it wise to seek a large practice for the sake of prestige.

In other words, the traditions of the medical guild antedated modern research and only slowly adapted themselves to it. Hence even the more original physicians rarely devoted much time to medical science, giving themselves rather to the related art of medical practice. Comte summed up the situation, early in the nineteenth century, in observing that the prospects for medical science were as dim as they would have been in astronomy, if all research in *that* science had been left to the sea captains[6].

One may conclude that, although medical science shared in advantages enjoyed by science at large, it also was handicapped by certain unsatisfactory relationships with the cultural environment. Since this lack of *rapport* was more or less peculiar to medicine, much of the relative lag in this field between 1600 and 1800 may be ascribed to it. But the slow pace of medical progress may also be blamed in part on the other major factor; that is, on the internal nature and logic of medical science as such.

In the first place, biologic phenomena in general—above the level of simple description—were in a sense more complex than were the physical. This was apparently not fully realized in the seventeenth century when, encouraged by the success of dynamics and influenced by the concept of the "animal machine", there was no little enthusiasm for an experimental and quantitative study of the bodies of men and animals. But although the first results were encouraging, as in the discovery of the circulation of the blood, the iatrophysical and iatrochemical schools were largely bogged down by 1700 in a morass of obscure phenomena and conflicting specu-

lations. It was easier in physics than it was in physiology, to isolate problems which could be solved in terms of the knowledge and techniques then available. Hence the zeal of 1700 for quantitative concepts and procedures in medicine, however sound and prophetic in principle, was of small avail at the time.

One may pause here to inquire whether this outcome really involved anything more than just another case of adjusting medicine to the surrounding culture—in this case, to the other sciences. For if the human body was simply an "animal machine"—a matter of controversy between vitalists and materialists—then medicine just called for the application of physical science to this body. And, in that case, medical sciences could only be expected to advance in the wake of the physical.

That there was some truth in this will hardly be denied. Biophysics presupposes an adequate physics, biochemistry an adequate chemistry. But this truism was not so applicable to the medicine of 1700–1850 as it is to that of the present time. This is because much of the significant research of the earlier period was done in pathologic anatomy and related clinical problems, and these fields made little use of either chemistry or physics. Indeed, they usually involved only simple observation, without benefit of the experimental and quantitative methods already taken for granted in the physical disciplines.

In other words, a great part of medical research prior to 1850 was still in the descriptive stage, even as was that in other biologic sciences. But although botanic and zoologic taxonomy presented real difficulties, these were relatively simple in comparison with the complexities and confusions associated with the taxonomic stage of medical developments. It is when one considers this phase of the story that he becomes more aware of problems inherent in medicine as such. As certain of these problems emerged, solutions were attempted along the lines of an internal logic but within limits set—at times—by external circumstance.

Medical thought was in a confused state during the eighteenth century. There was some enthusiasm for Baconian induction in general, and for the Greek clinical tradition in particular. But the Greek heritage also involved a theoretic, generalized pathology which could be neither proved nor disproved by the knowledge and procedures then available. Learned physicians felt it necessary to defend this "rational" pathologic theory, along with more tangible elements in medical knowledge, against the scepticism of the "mere empirics"—a controversy which had likewise been inherited from classical times[7].

The rationalists repeated the ancient query: what basic, bodily condition or conditions are involved in illness? They also, in most cases, echoed one of the chief Greek replies; namely, that illness was a condition in which the body fluids or humors (blood, bile, etc.) were impure or out of balance. Once accepted, this theory led logically to a therapy of bleeding and other depletion procedures, intended to eliminate impurities or to restore balance in the "general state of the system". Names had long been given to the more obvious "clinical pictures" (smallpox, great pox, etc.), but there was little interest in disease identification. Since it was the state of the body which the physician treated, and as this seemed much the same in each stage of illness regardless of any names employed, why bother about any exact diagnosis?[8].

This view is not in itself to be lightly dismissed, as has been the wont of medical critics during the past century. It involved some shrewd insights or at least inspired guesses; indeed, it may present us with one of the basic alternatives in outlook to which pathology will from time to time return. But the point here is that the ancient, humoral pathology, as still accepted in the 1700's, was both vague and unconfirmed. Professional discussions of its validity, or of that of opposing theories[9], were reminiscent of the doctrinal disputes of an earlier scholasticism; rather than of an effort to verify in the manner already established in the physical sciences.

It is true that some physicians defended the speculative pathologies by appealing to the clinical evidence. It was said, or at least implied, that if good results followed upon the application of these theories, then the theories themselves must be sound. Viewed simply in terms of the internal logic—that is, reasoning as one could have done *if* medicine had operated in a social vacuum—the absurdity of this reasoning would have been apparent. *Post hoc, ergo propter hoc* has always been an easy target for those exposing logical fallacies. But the fact is that medicine moved in the rather dense atmosphere of human hopes and fears already noted. Men wished for obvious reasons to believe that medical theories were sound, and under these circumstances were not too critical of the means employed for verification.

There was long no escape from pathologic speculation (with its unhappy encouragement of heroic practice) other than a resort to more or less "crude empiricism". But the empirics, also, had labored long and to no great avail. Scornful of speculation and claiming to be the true Baconians, they sought by unsystematic trial-and-error to discover means of amelioration, prevention, or cure. Even if one assumes that most drugs then

known had no rational background—which was not necessarily true—the cumulative achievements of empiricists were hardly impressive. These consisted, apart from the precepts of personal hygiene, of some helpful but dangerous and superficial surgery, and of awesome but largely useless pharmacopoeias. At best, a few drugs of ameliorative value and (by 1700) two actual specifics were known—mercury against syphilis, and cinchona against malaria. And about 1721 the first empirical achievement in preventive medicine—smallpox inoculation—was introduced[10]. Limited as these achievements were, the record was rather impressive when compared with that of the so-called rational school. Yet the latter was correct in its basic assumption that in medicine, as in all sciences, progress based on principles would be more rapid than that based on blind trial-and-error.

What could these principles be? Since the purpose of physicians was to lessen illness, one would have expected *a priori* that it would be helpful to understand the essential nature of illness; and, if this was a state of the humors, to discover what factors caused this condition. For if causal factors were known, one could then seek rationally for means of avoiding or overcoming them. Most physicians, prior to 1800, had the vaguest notions on this score. They spoke, as had the Greeks, of unhygienic habits, of heredity, of poisons in airs and waters, of contagion, and of what now would be termed psychosomatic influences. A few were even convinced that infections could be traced to minute "insects" or animalculae. But these explanations were rarely verified in any exact manner.

As long as one general state of the body was assumed to underlie all illness, indeed, there was no great interest in causal factors (etiology) except in relation to prevention. For since illness was viewed as basically of the same nature in all cases, physicians focused their attention on this condition. What had originally caused the biliousness, the dropsy, or the fever, was not so important as was the question of how one dealt with such a condition once it had appeared. After all, it was for this curative function that physicians were desired in society. Here one encounters again a limiting social circumstance. Physicians did write at times on preventive hygiene, but laymen usually felt that this was a matter of folklore or common sense. By tradition—and this is still all too true—physicians were called in only to treat acute illness. And at that stage, etiology seemed to the humoralists to be largely an academic matter.

There was, however, another Greek tradition which taught a doctrine essentially different from that of humoralism. The so-called school of

Cnidos had held that there was no one pathologic state common to all illness. Rather were there many distinct diseases; from which it followed that there were many distinct causal factors—some or all of them specific for particular diseases. Means of prevention, cures, and prognoses were also likely to be of a specific nature. From this viewpoint, the first purpose of physicians was to discover these different diseases; for one could hardly seek the prevention or cure of a particular disease until this entity was itself identified.

From the time of Galen until the sixteenth century the humoralist tradition dominated medicine, while the Cnidian was recessive. Just why a few physicians then revived emphasis on the latter is not clear. Increasing knowledge of non-Galenic, Greek medical literature may have had something to do with it. There were always clinical phenomena, of course, which suggested differences in types of illness; and it is conceivable that a renewed attention to these differences resulted from a slow but pervasive improvement of observation in general. More definitely, it has been suggested that the discovery of a remedy which was helpful against *only one* type of illness, implied that this "clinical picture" must be distinct from all others. The most striking instance here was the discovery that cinchona bark was a specific for a certain type of fever (malaria), which made a deep impression on physicians during the seventeenth century[11].

Whatever the explanations, the doctrine of specificity was clearly revived and emphasized during the latter part of that century. The most sweeping presentation was that of the English clinician Sydenham, who held that diseases were as real and diverse as were species of plants and animals. Each disease entity had its own causes, its own natural history, and even —if these only could be found—its own cures[12]. The optimism implicit in this outlook has not always been fully appreciated, but it must thereafter have had an increasingly stimulating impact upon medical thought. Instead of continued dependence on the shopworn and static doctrines of humoralism, those who accepted the concept of specificity could envisage new and promising discoveries all along the advancing front of medicine.

This optimism, although it may have originated in Greek ideas and was, in any case, internal to medical thinking, was encouraged because it harmonized with—and perhaps contributed to—the general optimism of the Enlightenment era. More than this, and here again is the interplay between internal logic and cultural environment, the subsequent triumphs of the specificity concept were made possible in part by developments

outside of medicine—especially by independent advances in other sciences.

This is not to claim that it was easy at first to identify specific diseases, to say nothing of finding specific cures for the same. It was difficult even to determine what criteria of identification could be employed. Sydenham and his successors defined disease entities largely by symptoms—a procedure which had always been vaguely followed in giving names to different patterns of illness. But when attempts were made to do this in a more systematic and exact manner, the effort bogged down in the multiplicity of symptoms and their combinations. The nosology texts of the later 1700's listed and classified almost two thousand so-called diseases, but these lists involved little more than names for that number of symptom combinations. So confusing did this situation become that some medical leaders maintained, or returned to, the view that there was only one underlying pathologic state in all forms of illness[13].

Neither an endless list of names, on the one hand, nor pathologic speculation on the other, could ever have stimulated much further research. The physician who believed there was only one underlying pathology—with some one basic treatment deduced therefrom—already knew all the answers. Why bother with more investigations? No matter how encouraging the cultural environment, medical progress would have been almost impossible as long as these internal convictions had been maintained.

Fortunately, the energy imparted by the concept of specificity carried medicine over this barrier of nosographical confusion. During the very era when symptoms alone were proving to be inadequate for disease identification, a second criterion emerged from medical research. The clue to this was found in the study of human anatomy, which had first been pursued in classical Alexandria, was undertaken once more in late medieval Europe, and was finally brought to a flourishing state during the Italian Renaissance. As research in normal anatomy expanded, it led by an almost inevitable, internal logic to a study of morbid anatomy as well.

The view that there was a relationship between structural changes in the body and illness was first expressed in classical Alexandria, but was thereafter largely lost to sight during the long dominance of generalized humoralism. Perhaps the possibilities were never entirely forgotten in the more obvious instances; for example, it was suggested even in medieval autopsies that crude obstructions might occasion illness. But normal, gross anatomy had first to be carefully investigated (1500–1750) before its implications for morbid anatomy could be demonstrated.

It has recently been pointed out by Temkin that surgeons played a

significant rôle in drawing attention to the relationships between structural changes and disease. Surgeons, in the nature of the case, had always dealt with structural conditions. They necessarily "had to rely on physical signs in their diagnosis and had to correlate the clinical picture to structural changes". These procedures were followed more effectively as surgery improved, once it was provided with a sound anatomical basis. It is true that surgeons usually dealt with injuries on the surface, rather than with those hidden within the body (lesions); but all that was needed in order to provide a complete structural pathology, was to transfer surgical attitudes *re* superficial injuries to internal injuries as well.

This transfer was delayed by lack of professional contacts between European surgeons and physicians—an environmental factor—but traditional barriers between the two guilds were partly overcome in certain countries even before 1750. As a result, some leading physicians were already familiar with surgical outlooks at that time. There is contemporary testimony by internists that it was this familiarity which finally encouraged them to think in terms of a structural pathology. The latter concept was first systematically presented in the work of Morgagni (1761), who was not a surgeon; but it was ignored for decades thereafter by most internists. Not until surgeons and physicians were brought into close association in the Paris school of about 1800, did the localized, structural pathology become dominant[14].

It was realized, after this time, that a correlation of *ante mortem* symptoms with *post mortem* pathologic data could reveal disease patterns which were more clear-cut and distinctive than were those composed of symptoms alone. A great impetus was thereby given, not only to autopsy studies, but also to the improvement of clinical investigations. As has often been said, physicians prior to 1800 *observed* their patients; thereafter, they began to *examine* them. The introduction of improved research methods in clinical medicine subsequently owed much to the cultural environment, in terms of mathematics and physics; as when the former made statistics available, and the latter (in association with technology) produced microscopes and other 'scopes which followed. But even more basic was the internal transformation wrought when physicians came to *look* for localized pathology; as is indicated by the fact that their first new instruments of observation (the hand, in percussion, and the stethoscope) were developed empirically without aid from contemporary physics or technology.

Between 1800 and 1850, rapid progress was made in the identification

of many diseases as still generally recognized, in terms of the correlation of clinical and pathologic data. In the place of the old humoral theories, or even of vague symptomatic notions like "inflammation of the chest" or "peri-pneumonia", there appeared such relatively specific concepts as "pneumonia" and "bronchitis". In the place of confused, symptomatic notions of various "fevers" (intermittent, continuous, remittent, etc.), there emerged the concepts of typhus, typhoid, malaria, and the like[15].

In consequence medical research was ready, by about 1850, to undertake the next step which Sydenham had long before envisaged; that is, to seek out the causal factors and cures of these now-identified diseases. In so doing, moreover, it was aided by improved methods which could not have been employed prior to identification. Thus the microscope (an instrument known long before this) could not have been used in a search for specific pathogenic organisms until the diseases to which these were related were clearly recognized. Pasteur could never have found organisms which were causal factors in such vaguely-conceived conditions as "biliousness", or "inflammation of the chest". In like manner, Louis could never have introduced clinical statistics as a check on therapeutic procedures, until he had first known the diseases with which he was dealing.

Up to this point, medical opinion had varied on the somewhat metaphysical question concerning the ultimate nature of diseases. Were they, as Sydenham held, objective entities—even as plant or animal species? Were diseases something real and outside of men, which invaded their bodies—a notion reminiscent of ancient, demoniac lore? There was considerable resistence to this "ontologic" concept, even among pathologists of the later nineteenth century, who tended to think of disease in a nominalistic manner as simply a form of bodily response to certain stimuli. Instead of viewing this response as involving the bodily "system" as a whole, however, they now thought of response in particular parts—in organs, tissues, or cells, depending on the historic stage of research involved.

When, after 1870, specific, pathogenic micro-organisms were discovered, bacteriologists at first viewed them as solely responsible for the related infections. And as long as typhoid bacilli were thought of as *the* cause of typhoid fever, it was easy to think of this disease as an entity—incarnate in the bacilli, so to speak, and loose in the community. Subsequent developments in immunology and other fields reduced the pathogenic

organisms to the status of merely "*a* causal factor", however, and the concept of disease as bodily response has again become dominant. All of this relates in part to internal developments in medicine, but it was also conditioned by the philosophic perspectives of medical scientists. Hence one should turn again, at this point, to the interaction of the internal developments with the surrounding cultural environment of the nineteenth century.

Each of the major philosophic outlooks of the eighteenth and nineteenth centuries had its implications for medical thought, despite the long-run trend toward divorcing modern science from metaphysics. Both philosophic empiricism and materialism encouraged scientific research, as did the subsequent development of Comte's positivism. The relationship between these "schools" and medical thought was at times quite definite, as when the *idéologues* of Paris encouraged, about 1800, the very program of objective clinical and pathologic research which has been mentioned[16]. The influence exerted by German idealism after 1815 was more complex and obscure. There is no doubt that the *Naturphilosophie* encouraged some return to grandiose speculation in pathology and related fields, especially in Germany between that date and about 1840. But there were only minor responses to this in certain other Western countries, as in the United States; and, in any case, the more extreme versions of this outlook were abandoned even in Germany thereafter. Some of the more subtle implications of the *Naturphilosophie*, moreover, may actually have had value for the great flowering of German research which then ensued[17].

More apparent than the influence of philosophy, was that exerted by social conditions and outlooks upon medicine during the nineteenth century. Certain aspects of this were favorable: the increase of wealth and of urban population resulting from the industrial revolution eventually benefited scientific institutions in many obvious ways. More specifically, the growth of cities, cheaper printing, and improved transportation facilitated the development of medical societies, research institutes, libraries, publications, and the like. Most important for medicine was the evolution of the large hospital—a product of social pressures—from a custodial institution into the type of research center needed for the clinical and pathologic studies of the times.

It need hardly be added that the striking progress made in other natural sciences during the nineteenth century proved of major advantage to medicine. This story is too well known to need repetition here; but it

may be noted again that the impact of the other sciences did not become obvious until after about 1850. This was not only because progress in these became more rapid thereafter; but also because medicine itself had first to go through the taxonomic and other stages mentioned, before it could fully avail itself of contributions from related fields.

On the other hand, there were certain aspects of social change which were less favorable for medicine. Or, to be more exact, the interplay between certain aspects of internal medical developments and a changing society was less favorable. Consider, for example, the social reactions of 1825–1875 to the expanding program of clinical and pathologic research. As already noted, it is difficult to see how basic progress in medicine could ever have been achieved except along this line. Yet the program was centered for more than fifty years on the identification of diseases, rather than on their prevention or cure. Research men were so preoccupied with this "pure" research, which had no immediate prospect of utility, that they lost interest in therapy. Moreover, their more critical temper led them to discard the older remedies, at a time when there was as yet little with which to replace them. A spirit of "medical nihilism" pervaded the best centers.

In so far as this nihilism became known to the public, it was not calculated to inspire confidence in medical practice. There is indeed evidence that this period, which we can now see was of great promise in medicine, was one in which the public had the least confidence in regular practice. One of the indications of this was the proliferation of rival medical sects, such as homeopathy, hydrotherapy, "the botanic system" (Thomsonianism), and so on. These sects preserved the old thesis of a single, generalized pathologic state, or of some single scheme of treatment, long after these over-simple formulae had been repudiated in regular medicine. Then they promised the cures which the more candid regular physicians no longer believed possible, and so appealed to a public which was often in dire need of such assurances[18].

More serious than popular doubts, moreover, was the danger that neither philanthropists nor governments were likely to support a field having little apparent utility. Modern science had early been hailed as a means to acquiring "useful knowledge"; and if this was indeed its major purpose, why assist it when it failed to serve that end?

The answer to this query was not a simple one. Actually, little direct private or governmental aid was extended to pure research in medicine or biology prior to 1860. But in relatively aristocratic countries not yet

dominated by industrialism, science continued to benefit from the deference long accorded to learning as such. This can be best observed in the prestige enjoyed by German or other Continental professors, and the support extended to them by their respective governments. It even became a matter of pride with some men that their research had no relation to "mere utility".

In relatively democratic countries, where business men became increasingly influential, the middle classes continued to encourage the pursuit of useful knowledge. Conversely, they had little desire to support basic research. This attitude could be observed to some degree in England, but found its extreme expression in the United States, where a "practical" people saw little reason for supporting the "idle curiosity" of pathologists or other pure scientists. It is hardly an exaggeration to say that, for most Americans, the word science connoted simply "applied science" or technology[19].

It is suggestive that in those countries where science was highly regarded for its own sake, there were notable achievements during the nineteenth century. This can be well observed in medicine, where the preëminence of French and German pathologists was widely recognized. Even more striking was the manner in which the French and Germans dominated bacteriology, as this field emerged after 1875. At the other extreme, again, was the experience of the United States, where—despite individual exceptions—the record in basic medical research was a negligible one. Had the matter been left to this country, it is unlikely that "modern medicine" as we understand it would ever have evolved. At best, the process would have taken a much longer time. Hence we have the paradox that, in pursuing immediately practical goals, the Americans proved in the long run to be a quite impractical people[20].

The fact that technology made rapid progress in the United States might conceivably have led to parallel advances in basic science there. Certainly there are circumstances in which one can observe technology stimulating science, as well as *vice versa*[21]. This was true even in medicine; for example, when knowledge of brewing and fermentation played a rôle in Pasteur's investigations. But whatever was true in special cases, the American story certainly indicates that applied science and technology *may* be successfully cultivated on a large scale without any major benefit to basic science. It also suggests that an exclusive devotion to applied science may even interfere with, or at least divert attention from, basic science. Americans were quite successful at times in applied medical

science, as in the introduction of anesthesia and in other contributions to surgery. Yet these achievements did little, prior to 1900, to stimulate basic research in medicine as a whole.

During the present century, one of the most striking internal trends in medical science has been the partial return to a generalized pathology. Beginning with a limited revival of humoral pathologic concepts (as in immunology and endocrinology), this trend was extended (in terms of nutritional research, psychosomatic studies, and so on) still further in the direction of a generalized pathology. Much of this involved simply the superimposing on structural pathology of more general concepts. But in the recent development of the sulfa-drugs and antibiotics, and more especially of cortisone and ACTH therapy, a more complete return to the outlook of a generalized, systemic pathology has been suggested. The first reaction seems to have been the thought that if certain drugs could "cure" many apparently distinct diseases, all of the latter must really have some underlying pathology in common; i. e. they are not really specific at all.

One notes here, again, what a potent influence pharmacology has apparently exerted at certain key points on the course of medical thought. Just as the discovery of a specific drug in the seventeenth century may have then revived the idea of disease specificity, so now the discovery of non-specific drugs may revive the concept of non-specific disease processes. To some contemporary medical men, the latter outlook seems "an entirely new concept"; which is natural enough in case their perspective is limited to the last hundred years[22]. The historical background, however, provides protection against such extreme awe for the accomplishments of our own generation.

The clinical evidence, as well as the historical, also suggests caution here. Apparently, in the case of acute infections, the new drugs may remove or suppress the symptoms without eliminating the disease process. This implies that the concept of specificity will continue to be useful in this area, even though there will be no such exclusive dependence on it as was the case only a generation ago. The results of cortisone and ACTH therapy in chronic disease have been more encouraging, even though actual cures have been rarely if ever involved; but it may be that observations have not yet been sufficiently prolonged to be certain of the conclusions.

Of course, if a drug which is literally a "cure-all" is ever discovered, we would then probably return to the notion of one underlying pathologic

state; but no such idyllic prospect looms before us at present. Meantime, the unexpected results of cortisone therapy raise some interesting questions; for example, are we to conceive of a disease as something quite distinct from its symptoms? But, if so, what are the underlying pathologic processes involved, other than merely some sort of internal "symptoms?" The concept of disease, apparently so obvious, actually remains a rather baffling one.

These recent internal trends in medical research have no obvious relationship to the social environment, other than to increase public confidence in medical practice. But much confidence had already been revived by striking progress all along the line in medicine during the past seventy-five years. The social implications or interactions of this general, internal progress were complex. The technical advances which made the public seek medical care, for example, also made that commodity more expensive; so that the more this care was desired, the less people could afford it. Out of this situation, in part, has grown the demand for government health insurance systems. And these systems, in turn, are likely to influence medical research as well as practice, for either good or ill.

The revival of confidence in medicine after 1870 had far-reaching implications, not only for medical care but also for the support of medical research. Outside of English-speaking lands, nearly all medical schools were located in state-supported universities; and ministries of education gradually increased the funds available to these schools as well as to other scientific faculties. In order to initiate new specialized programs, moreover, special research institutes were set up for outstanding scientists—those established for Pasteur, Ehrlich, and Koch come readily to mind. Some of these were state-supported, others were given private endowments; some were autonomous institutions, others became units within universities.

In English-speaking countries the very emphasis upon the practical, which had hitherto inhibited basic investigations, now encouraged them—once it became clear that these really promised utility of a new order. This was particularly fortunate because Britain and the United States were wealthy nations, in an age when research was becoming ever more expensive. In the United States before 1900, medicine had played the rôle of a neglected Cinderella; but thereafter it became the chief beneficiary of great private foundations. Beginning with the 1920's in Britain, and the '30's in the States, medicine—as well as other sciences—also received increasing support from governmental sources[23].

The recent trend toward state aid, which may be interpreted as a reaction against earlier *laissez-faire* attitudes toward science, was carried much further in fascist and communist societies. In the latter, state support of medical and other scientific research has been accompanied by state control; and in the Western countries fear of such control has now dramatized the whole theme of the relations between science and society[24].

One may close the discussion with this brief note on recent developments, which are obviously of great significance. Both the internal developments within medicine, and social transformations in its environment, now proceed at an accelerated pace. The interplay between the two is as inevitable now as in times past, but the actual components and results of the process are changing rapidly.

## NOTES

1. E. g., "foci of interest" are here listed as relating to internal logic, but in certain cases these may be the result of environmental circumstances.
2. Sufficient literature has appeared on this theme, and on the "sociology of knowledge", as to elicit analyses and bibliographies. For the European literature, see, for example, Robert Merton, *The Sociology of Knowledge*, Isis, Vol. 27 (Nov., 1937), 493 ff. For the more recent literature, there are a number of bibliographies in English; e. g., M. C. Leikind, *The Social Impact of Science* (Government Printing Office, Washington, D. C., 1945).
3. It is, of course, entirely justifiable to present the history of scientific developments primarily in relation to the surrounding culture, as in Merle Curti's able work *The Growth of American Thought* (New York, 1943). But there is always some danger of misinterpretation, unless the limitations of this frame of reference are clearly stated; see, e. g., the author's review of this study in the American Historical Review, 49 (July, 1944), 732 ff.
4. "Medicine" is used throughout here in a broad sense, to include medical practice and institutions, the public health, etc., as well as "the medical sciences".
5. It should be emphasized that "environment" is also used here in a broad sense. It refers not only to such changing elements as are suggested by terms like "the Enlightenment" or "the romantic era", but also to the whole complex of attitudes, traditions, institutions etc., which were inherited from the past.
6. Auguste Comte, *Cours de Philosophie Positive*, III (Paris, 1908; first ed., 1830), 148 ff. Comte, moreover, was doubtless thinking of the European situation, which was not so extreme as that in the United States.
7. Galen's comments, e. g., are given in A. J. Brock, *Greek Medicine* (London, 1929), 130 ff. On the eighteenth century form of the controversy, see R. H. Shryock, *Development of Modern Medicine* (London, 1948), Chapter 2.
8. Knud Faber has noted the expression of this view which is found in the concluding statements of the Hippocratic text on *Prognostics*; see his *Thomas Sydenham, der*

*englische Hippocrates u. die Krankheitsbegriffe der Renaissance*, Münchener Med. Wochenschrift, no. 1 (1932), 29.

9. Somewhat analogous to the humoral tradition was that of the rival tension and laxity (*strictum et laxum*) pathology, which usually related to assumed conditions in the solid parts (solidism) of the nervous or vascular systems. Both these traditions involved speculative, generalized pathology.
10. Unless one also includes the use of citrus juice against scurvy in this category—dates here are difficult to determine.
11. Knud Faber, *op. cit.*
12. Benjamin Rush, (Ed.), *Works of Thomas Sydenham* ... (Philadelphia, 1809), xxiv ff.
13. E. g., Dr. Benjamin Rush of Philadelphia announced about 1800, as a "new" theory, that there was only one pathologic state. He was then hailed by many as having brought order out of the chaos of the nosologies. A long poem to this effect is preserved in his papers in that city.
14. Owsei Temkin, *The Role of Surgery in the Rise of Modern Medical Thought*, Bulletin of the History of Medicine, 25 (May–June, 1951), 248 ff.
15. The evolution of these concepts can be traced by comparing old "bills of mortality" with the later lists of "causes of death".
16. George Rosen, *The Philosophy of Ideology and the Emergence of Modern Medicine in France*, Bulletin of the History of Medicine, 20 (July, 1946), 328 ff.
17. See, e. g., Walter Pagel, *The Speculative Basis of Modern Pathology. Jahn, Virchow and the Philosophy of Pathology*, Bulletin of the History of Medicine, 18 (June, 1945) 1 ff.
18. R. H. Shryock, *Quackery and Sectarianism in American Medicine*, The Scalpel, May, 1949.
19. R. H. Shryock, *American Indifference to Basic Science During the Nineteenth Century*, Archives Internationales d'Histoire des Sciences, No. 5 (Oct., 1948), 50 ff.
20. Tocqueville, in analyzing this situation in 1835, thought that Americans would have developed basic science if they had lacked European aid. But he cited Chinese civilization as having failed to do this under those circumstances. (*Democracy in America*, New York, 1904, vol. II, 518). One also thinks of the analogy of the practical Romans.
21. See, e. g., S. Lilley's interesting analysis of the relationship between technology and the laws of thermodynamics, *Social Aspects of the History of Science*, Archives Internationales d'Histoire des Sciences, no. 6 (Jan., 1949), 376 ff.
22. See, e. g., the statements by Dr. J. S. L. Browne of McGill University that: "... the symptoms of tuberculosis are the manifestations of the injury inflicted by the tubercle bacilli. But underlying them is the *general* response of the body to damage, *any damage* ... This philosophical point of view greatly alters our concept of disease ... And this idea ... is completely at variance with the older views of scientific medicine". Quoted by George W. Grey, *Cortisone and ACTH*. Scientific American, vol. 182 (March, 1950), 35, 36. Italics are those of the author.
23. R. H. Shryock, *American Medical Research: Past and Present* (New York, 1947), Chapts. 4, 8.
24. *Cf.* J. D. Bernal, *Social Function of Science* (New York, 1939), Chapt. 1; and M. Polanyi, *The Contempt of Freedom: The Russian Experiment and After* (London, 1940).

# THE DISCOVERY OF NEPTUNE

by

A. Pannekoek*

## I

Of all the astronomic discoveries of the nineteenth century the discovery of the planet Neptune may not be the most important, but it is certainly the best known, having caused the greatest sensation by its dramatic developments. The course of events leading up to the discovery has often been described and its general outlines are well known. William Herschel discovered Uranus in 1781 and in the succeeding years its orbit was calculated. These calculations showed that Uranus had been observed as a fixed star on several previous occasions in the eighteenth century (by Flamsteed in 1690, 1712 and 1715; by Bradley in 1753; by Mayer in 1756; by Le Monnier in 1750, 1764, 1768, 1769 and 1771). A painstaking investigation of its motion, taking into account the perturbations caused by other planets, especially those due to Jupiter and Saturn, was given in the tables of Alex. Bouvard in 1821 and showed that the early observations did not harmonize with those made after the discovery. Moreover, in the following years Uranus appeared to deviate more and more from the orbit calculated from the later observations; in 1835 the deviation was already 30″ and in 1841 as much as 70″. Airy, the director of Greenwich Observatory, found from observations in 1833–35 that, in addition, the radius vector (i. e. the distance from planet to the sun) did not fit the calculated orbit. In the eighteen-thirties astronomers gradually became convinced that these discrepancies were caused by the attraction of an unknown disturbing body, that of a more remote planet. Out of this supposition rose the problem: was it possible from a knowledge of the perturbations in the motion of Uranus to derive the position and orbit of the unknown body? In 1836 Mary Sommerville expressed it thus: "The discrepancies

* Em. Professor of Astronomy at the University of Amsterdam.

*Centaurus* 1953: 3: 126–137

may reveal the existence, even mass and orbit of a body placed for ever beyond our vision".

The problem had already intrigued Bessel, the recognized master of observational astronomy of those days. In 1840 at a lecture in Königsberg he announced that one of his pupils, Flemming, had already started an attempt to solve it. But the latter died after having finished some preliminary calculations. Bessel then intended to treat the problem himself but he was prevented from doing so by too much other work and he died in 1846. In 1843 John Couch Adams, a student of mathematics at Cambridge, tackled the problem. In September 1845 he communicated his first results, regarding the orbit and position of the unknown planet, to Challis, director of the Cambridge Observatory, who informed Airy of them on the 22nd September. After some attempts to get into touch with Airy had failed by mischance, Adams sent Airy new and improved results according to which the mean longitude of the planet should have been 325°7′. Airy replied with a query to know whether the discrepancies in the radius vector which he had found were also explained by Adams' results. If Adams had answered this question (which he could quite easily have done) more attention would probably have been paid to the matter. But Adams did not answer and so Airy dropped it altogether, doubting the seriousness of the young man's work. Subsequent English writers, angry that the honour of this discovery should have escaped their country in this way, have described the course of events as incomprehensible. Yet it becomes understandable when Adams' extremely modest personality is considered; shy people do not like to force themselves forward and they tend to withdraw when they perceive distrust rather than encouragement.

In 1845 Eugène Bouvard, who was continuing his uncle's work, published new tables of Uranus' motion which showed equally strong discrepancies. In the same year Arago suggested to Leverrier that he should tackle the problem of Uranus. Leverrier, a capable, self-confident and acute theoretician, had already in 1840 made his name by an investigation of the motion of Mercury. In order to give his work a sound foundation he immediately began to investigate thoroughly the observed motion of Uranus, to reduce the observations carefully once more and to calculate the perturbations more completely and more precisely than had Bouvard. The results were submitted to the Paris Academy in November 1845. Then, on June 1st, 1846, there followed a communication concerning the orbit and mass of the unknown body and the place where it ought to be found—in Capricorn at a longitude about 325° in the ecliptic. It should

be said that the determination of the path of the disturbing planet as a completely unknown entity would have been too difficult a problem, almost impossible to solve. With such a large number of unknowns each of them separately would have been so ill-defined that a solution could not have been of any practical value. Therefore Leverrier's initial assumption, as with Adams, was the validity of Titius' and Bode's rule connecting the distances between the planets and the sun. Thus he assumed the semi-major-axis of the orbit to be 38, twice that of Uranus, corresponding to a period of revolution of 230 years. There then remained as unknowns only the magnitude of the eccentricity of the orbit, and the direction, mass and longitude (i. e. position in its orbit) of the planet. The latter was the most important as the aim of the calculations was mainly to provide the observers with the information necessary to discover the planet.

For the time being this paper had no other result than that Airy noticed its conformity with Adams' results and began now to realize the value and substance of the latter. He suggested to Challis a plan to trace the planet. They realized that it would be an extensive labour because of the lack of good, complete, star-maps. To find from among hundreds of telescopic stars in a vast environment the one that is moving, they all had to be catalogued and later to be verified. Challis started his observations on a large scale on July 29th and continued them in the first weeks of August; however, being kept busy by urgent calculations of comets, he postponed the reduction and comparison of the results. Otherwise he would certainly have discovered the planet as the moving star which he had observed on August 4th and 12th.

Since then Challis has been severely criticized, especially by his fellow countrymen, for his earlier unbelief and his later lack of perseverance. But it must be remembered that nevertheless Challis was the only man in Europe who set to work. No astronomers either in Paris or in any other country took up the idea of tracing the planet in the place indicated by Leverrier. It was felt that the job would be difficult and cumbersome, without any certainty of result, and it would be one for which other work would have to be sacrificed. There was no positive distrust of the correctness of the calculations but, equally, there was no conviction that a real visible body would correspond to the object of the theoretical calculations.

In the meantime Leverrier had continued his calculations and, on August 31st 1846, he communicated to the Academy the elements of the orbit of the unknown planet together with their uncertainties calculated according to the method of least squares. The semi-major-axis was now somewhat

smaller (36·15), the eccentricity was 0·1076 with perihelion at 284°15′ and the true heliocentric longitude on January 1st 1847 became 326°32′±5°; the mass was calculated to be 1/9300 of the sun's mass. By assuming its density to be the same as that of Uranus Leverrier could estimate the angular diameter to be 3″, large enough to distinguish the planet from the ordinary stars as a noticeable disk. Adams, too, had been continuing and correcting his calculations by assuming a rather smaller value (37·25) for the semi-major-axis. He thus found an eccentricity 0·1206 with perihelion at 299°, a mass 1/6600 that of the sun and the true longitude on January 1st 1847 of 329°57′. All the observations of Uranus in the eighteenth century were now satisfactorily accounted for; only the observation of 1690 showed a discrepancy of 50″. All the observations since 1780 agreed to within one or two seconds with the calculations except those of the most recent years which gave a deviation rising to 10″. Adams noticed that a still further decrease in the assumed semi-major-axis (down to 33·5) would remove these residual discrepancies. When he sent these new results to Airy in September 1846, he also gave a reply to Airy's former query by calculating the values of the radius vector.

Leverrier, knowing nothing of Challis's observations, now became impatient because no efforts had been made by the astronomers to trace the predicted planet in the place indicated by him. On September 18th he wrote to Dr. Galle, observer at the Berlin Observatory, to thank him for a reprint and at the same time to ask him to explore thoroughly with his large telescope the stars in the indicated region to see whether one of them was distinguished by a disk of 3″ diameter. The very day he received this letter, the 23rd of September, Galle took the job in hand; his assistant d'Arrest, then a student of astronomy, drew his attention to the fact that they had a printed star-map of this very region (hora XXI of the maps of the Berlin Academy, printed but not yet distributed to the other observatories). When it was fetched and compared with the sky by the two astronomers they soon found a star of the eighth magnitude which was missing from the map. It was the looked-for planet at a distance of less than one degree from the predicted position. Observations during the following night showed a displacement of 1′ in accordance with expectation.

## II

When the news of the discovery spread through Europe, there was an outburst of enthusiasm among scientists and the educated public. The

power and certainty of science were demonstrated as never before. An unknown, unseen, heavenly body had been discovered with pencil and paper at the mathematician's desk. It was a brilliant proof of Newton's Law of Universal Gravitation. When the observed motion of Uranus was found not to fit the calculated orbit, it had been claimed that there must exist a disturbing body. And it did exist. Leverrier's name was on everybody's lips; the press praised his discovery as the greatest event of the century. Congratulations and homage poured in on him from governments and learned societies in France and abroad. The French people shared in his glory; whenever his genius was applauded it was added that only France—already famous for her numerous eminent mathematicians—could have brought forth such genius.

This enthusiasm was not a measure of the discovery's importance for the progress of science. The authors of far more important discoveries which determined the subsequent development of science were accorded far less honour or even passed quite without notice. Neither was it the fact (as has sometimes been stated in popular accounts) that Newton's Law was for the first time proved in practice by the discovery. Every planet following exactly its calculated path, every observation of a heavenly body whose orbit was calculated from the Law of Gravitation, was an equally conclusive proof of its truth. Surely there was a difference. Here was a triumph of science which was bound to impress even the uneducated public. But why was the judgement of ignorant laymen made the criterion of scientific acknowledgement?

We have to consider the part played by the discovery of Neptune in the spiritual and social struggle of the time. It was the period of the "Aufklärung", the "enlightenment", when the rising bourgeoisie was tearing itself away from traditional beliefs and finding a powerful substitute in Natural Science. Due to rapid industrialization and the development of large scale industry the bourgeoisie (the "middle class") became a more and more important force in society fighting everywhere for political power and for spiritual influence over the people, often with the slogan of Liberalism against Conservatism.

This struggle was now approaching its climax. In England it had been going on since the Revolution of 1640–60, and, after many stages of advance, the bourgeoisie had already reached a situation in which the Reform Act of 1832 and the repeal of the Corn Laws in 1846 left it as virtually the supreme power in the land. In France (where the first victories were won in 1789) and Holland (where the initial advances took place in

the 16th century national wars against Spain), the bourgeoisie had not yet gained power, but were to do so in 1848; while in both Germany and Italy the practical struggle was to start soon after with political consolidation into unified states.

In England the virtually complete victory of the merchants, industrialists and bankers before the discovery of Neptune created a special situation. But elsewhere, in this struggle against the ruling powers of the past—absolutism, aristocracy and Church—the bourgeoisie had to win the people to its side by propagating its new principles. Its aim was to refute the traditional doctrines on which the old world relied by means of the new scientific truth. Natural science was disseminated among the masses of the people in numbers of popular books. Scholars, scientists and university-trained intellectuals played the most important part in this work. They did not aim at transforming the masses into students of science but it was necessary that a belief in science should take the place of belief in church doctrine. Such a striking discovery as that of Neptune was the best thing that could have happened to further this aim of rousing conviction in the truth of science. Now the truth was demonstrated even to the ignoramuses. Hence the enthusiasm not only in the world of science but also among all educated people who sympathized with the new ideas. Hence homage to Leverrier, the happy discoverer and the pride of his countrymen.

In England where the bourgeoisie already felt its supremacy such a passionate struggle for scientific enlightenment against clerical tradition was hardly noticeable. This surely explains the calm indifference with which Adams' work was treated and passed over by the leading astronomers. Strangely enough, Airy did not mention Adams' work at all in his letter of June 1846 in which he congratulated Leverrier on his results and enquired about the perturbation of the radius vector of Uranus—a trivial query, easily settled by Leverrier. It seemed as if people in England tried to avoid any semblance of national pride; possibly this was a symptom of the cosmopolitan mentality, then very strong, directed towards free trade and world peace. Adams' work was mentioned only after the discovery in a paper by John Herschel in the journal *Athenaeum* and also in private letters. It was mentioned, however, in such cautious terms alongside a laudatory recognition of Leverrier's claims that Adams' merits were, in fact, undervalued. All the same, Herschel's paper fell like a cold shower on the boundless enthusiasm in Paris and there it evoked expressions of disbelief, indignation and rage, even suspicions of dishonesty.

This was understandable as the silence of the English astronomers while the work seemed an uncertain hypothesis, and their claims to a share in the honour and glory after the discovery, was indeed bound to arouse distrust. The idea, too, that a problem requiring Leverrier's superhuman genius should have been previously solved by an English student was a hard blow to the national pride of France. However, there was no escape when everything came to light; the honour had to be shared.

When, during the last months of 1846, the work of both investigators was published, it could be seen how differently they had tackled the problem. Adams had looked on it as a simple mathematical problem which he treated according to the ordinary rules. From the normal equations for all the unknowns—the elements of both the unperturbed Uranus orbit and that of the disturbing planet—the former are eliminated. The remaining unknowns (the longitude in the orbit, eccentricity, the longitude of perihelion and mass of the disturbing planet) are then calculated by successive approximations combining the results of the uninterrupted series of observations after 1780 with the scattered data from before that date. After this the same calculation is repeated for another assumed value for the dimensions of the orbit. All this is in a paper of only 31 pages, while Leverrier had regarded it as a difficult astronomical problem and his treatise run to 254 pages. Of these 150 were needed for a thorough revision of the whole theory of the perturbations of Uranus by the other planets so as to leave no uncertainty whatever, and for an equally thorough proof that it was in no way possible to represent Uranus' motion without recourse to an unknown disturbing body. In addition, the further work was extensive. It meant a cumbersome search to determine the position of the disturbing planet from the slowly varying deviations of Uranus. Sometimes it even looked as if the solution was quite fictitious since it led to a negative mass value. "I readily admit", he said, "that this happened to me at first; for a long time my investigations were held up by this difficulty"[1].

He succeeded in reaching a decision as to the location of the planet only by assuming 40 different positions evenly spaced around the ecliptic and carrying out the complete calculations for each assumed position. Then the position was limited more precisely by restricting the semi-major-axis between the limits 35·04 and 37·90. Thus the work of Leverrier gave the impression of being a laborious investigation made possible only by great skill and perseverance but consequently complete and conclusive. However, in the opinion of the theoretician P. A. Hansen of Gotha, author

of the theory of the moon, Adams' work was much more beautiful mathematically than that of Leverrier[2]; Airy, later on, expressed the same view. When Leverrier came in 1848 to be honoured, together with Adams, by the Royal Astronomical Society, he had already realized that he need not be ashamed of the partner with whom he had to share the honour. Adams was already beginning to be recognized as one of the prominent figures in the field of celestial mechanics. The situation seemed to contain all the elements necessary for fierce personal conflict; however, Adams' modesty and his complete lack of vainglory and ambition made a friendship possible based on an appreciation of each other's scientific merits. Leverrier was the man who completed with formidable perseverance and sagacity the gigantic work that settled the theory of all the large planets; Adams the man who, by deeply penetrating reasoning, was able to solve conclusively the most difficult and disputed problems of the theory.

The simultaneous appearance of two theoretical discoveries had a special significance for the scientific world. It showed that the great discovery was not a chance happening nor was it the achievement of a single genius; rather was it the natural fruit of the development of the whole of science.

## III

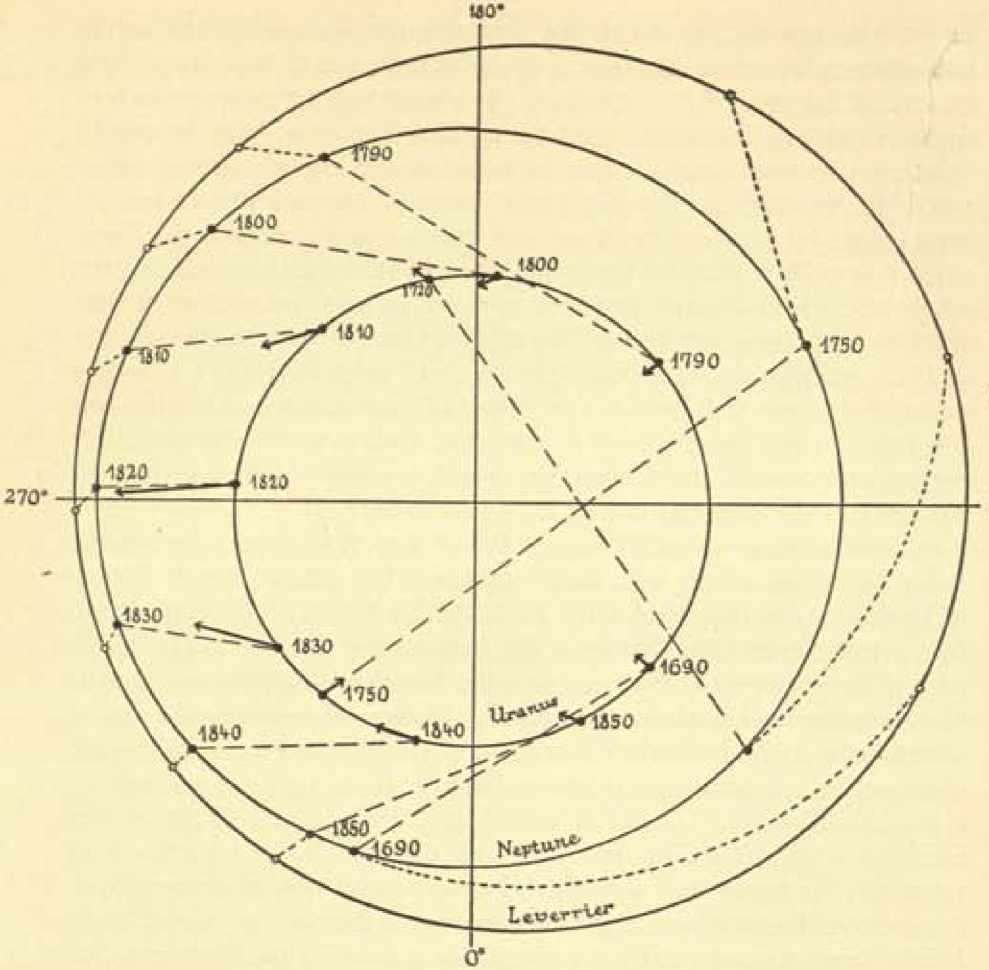
The difficulties and disagreements, however, were not yet ended for the astronomers. At the beginning of October 1846—as soon as a number of accurate observations of the new planet had been made—Adams calculated an approximate orbit from them and from those of Challis made on August 4th and 12th. This orbit gave a distance from the sun of 30 astronomical units, considerably smaller than the value accepted in the earlier calculations. This difference was shown even more convincingly by orbits calculated in February 1847 by Adams and others. By means of these latest orbits two old observations of Neptune made by Lalande, who had determined its position as a fixed star in 1795, were traced. Walker, in the United States, used this position to calculate a precise orbit of Neptune. He found a semi-major-axis of 30·04, far outside the limits given by Leverrier, and an eccentricity of only 0·0086.

This result caused no little consternation among the astronomers. For it seemed to show that the real orbit of Neptune was entirely different from that calculated independently, yet with almost complete agreement, by Leverrier and Adams. According to Leverrier the period of revolution

should have been 217 years; according to Walker it was only 165 years. Consequently, the real Neptune had, for the most part, been in totally different positions in space from those calculated from the predicted orbit. Therefore the attraction of Uranus should have been completely different. How, then, could the coincidence that the predicted and real positions were so close together in 1846, be explained? The American astronomer Peirce expressed the dilemma sharply by the sweeping statement: Neptune is not the planet whose orbit was calculated by Leverrier and Adams but a different one which happened to be in the same neighbourhood. Leverrier, when questioned in the Paris Academy, was irritated by a few superficial conclusions of the physicist Babinet and passed off the matter by saying that the calculated orbit satisfied all the requirements and by indicating at the same time much wider limits of error than before. But this did not solve the dilemma. Echoes of it appeared in the newspapers who made mock of the astronomers who had lost their new planet and of the fictitious Neptune. The European astronomers either kept quiet about the whole matter or, like Struve and Herschel, they rose to defend Leverrier against the "attacks" of Peirce and Walker. Not one of them felt the urge to investigate Neptune's orbit precisely.

All doubts were settled by the two American astronomers. Peirce and Walker first of all calculated Neptune's orbit as exactly as possible from all the observations taking into account all the perturbations; they then calculated the perturbation of Uranus caused by this real planet. Then it became clear to them that not only were all the observations of Uranus used by Leverrier and Adams represented equally well by both the real planet and the "predicted" one, but, in addition, the earliest observations of 1690 and those of more recent years all fitted the observed orbit exactly. Their further results also explained how this came about. As in many other mathematical problems the one tackled by Adams and Leverrier admitted of several solutions, all different but all capable of satisfying the data. The data, i. e. the deviations of Uranus' orbit, are not the perturbations themselves—if these had been known the solution would have been straightforward and simple—but the perturbations calculated from an orbit which was itself affected by them. The observational data could be resolved into these components (orbit and perturbation) in a variety of ways. Starting from an assumed mean distance from the sun of 38 Adams and Leverrier had found one solution whereas Neptune itself represented a different one.

The diagram may elucidate the situation. It shows the orbits of Uranus




and Neptune with their positions in 1690, 1720, 1750, 1790, 1800, etc. and the lines joining these positions. Outside is the second orbit calculated by Leverrier (August 1846) with the positions at the same instants. One sees that the calculated and true positions of Neptune are close together between 1800 and 1850 but far apart during the whole of the 18th century. As Adams and Leverrier started with too large an assumed orbit, they required a large eccentricity—with perihelion about 280°–300°—to make the calculated distances to the sun and to Uranus equal the true small values in those critical years. In fact, the calculations show that the greater the semi-major-axis of the assumed orbit, the greater its eccentricity has

to be. The amount, by which the distances are still too great, can be compensated by assuming a larger value for the mass of Neptune (1/9300 instead of the true value 1/19000). If the planet had not been discovered so soon and if Adams had carried out his intention of looking for a third solution with a still smaller orbit, he would have come even nearer to the truth. In the diagram the disturbing force of Neptune on Uranus is represented by arrows (the direct and indirect terms combined). From these it is evident that the perturbing forces are insignificant during the whole of the 18th century. They do, however, increase strongly after 1800 reaching a maximum in about 1820 (the two planets were in conjunction in 1822) and then decrease until they are small again after 1850. It is only during these years that there is a large mutual perturbation of Uranus and Neptune; outside this period it is negligible. Thus orbits which are widely separated for most of the time may still produce nearly equal perturbations because of their closeness during the sensitive period.

In Europe these investigations of Peirce and Walker did not receive much appreciation and were almost ignored. The astronomer F. Kaiser of Leiden viewed this unscientific attitude with surprise and disapproval. In a popular book, *The History of the Discovery of the Planets* (1851), he said of the discovery of Neptune that the European astronomers seemed "to have regarded it almost entirely as a means of inspiring the public with a belief in the perfection of science"; "they wanted to prove by that discovery such a perfection in their science as could never be reached by a product of man". "Therefore anything that could cast doubt on the accordance of prediction and fact was disputed with prejudice and rejected". "It seems that nobody in Europe dared risk an investigation the clear verdict of which might have disproved the beloved theory". "In North America people did not applaud the miracle of the discovery, but worked all the harder to make it serve the welfare of science".

From these remarks it appears clearly that not science itself but its social function ruled the attitude of scientists in Europe. When the direct brilliancy of the discovery had to make way for doubt and argument the feeling of triumph was lost and with it the propaganda effect of the discovery. When a prediction is not verified, it usually means for science a step forward toward new discoveries, but it means a set-back in its use in the social-cultural struggle. Thus the embarassed silence in Europe can be understood. In America this struggle was unknown. There was no established ruling power, there was no feudalism leaning on a powerful Church. The religion which the immigrants, often persecuted

dissenters, had brought from Europe was thoroughly democratic and they themselves were free citizens. Thus in America there was no need to fight that passionate struggle for social progress in which Natural Science, as a basis of a new spiritual culture, played such an important part in Europe in the middle of the nineteenth century.

## NOTES

1. J'avouerai sans peine que c'est ce qui m'est d'abord arrivé; longtemps j'ai été arrêté dans mes recherches par cette difficulté (p. 175).
2. Cf. F. Kaiser's narrative *Gotha en de Seeberg*, p. 33. (De Gids, 1848).

# SCIENCE, INDUSTRY AND SOCIETY IN THE NINETEENTH CENTURY

by

J. D. Bernal*

In the relations between technique and science the nineteenth century is the major period of transition. The great transformation of the farming and handicraft economy, which had endured with little radical change for three thousand years, into the large-scale highly mechanised agriculture and industry of today had already begun in the eighteenth century. It was to become fully conscious, with mass production and organised research, only in the twentieth century. We are still in the process of finding the appropriate social forms to match the new productive capacity, as the revolutions and wars of our time so tragically bear witness.

In the eighteenth century only the steam engine was there to show the kind of contribution that science could make to industry. The major advances of the industrial revolution, in the smelting of iron, in spinning and weaving, were achieved by practical workmen and owed little to science. By the twentieth century science was everywhere, and it was already clear that every significant technical change could only come as a result of scientific research. It was, however, in the nineteenth century, in the very middle of the great transformation, that the enduring link was forged between science and industry. How and why this happened, it will be the business of this essay to enquire.

The nineteenth century was first and foremost a century of expansion—expansion of production, expansion of population, expansion of trade, geographical expansion, expansion of enterprise, above all expansion of profits. Beginning with a small nucleus in Britain and France, industrial capitalism was to spread until at the end of the century the whole world was paying it tribute. It was during those hundred years that capitalism was to experience its greatest triumphs, and to approach, though not yet to notice, its incipient decline.

* Fellow of the Royal Society of London; Professor of Physics, Birkbeck College, University of London.

*Centaurus* 1953: 3: 138–165

In science, as in industry, the original impetus to the new advance—the second wave of the great renaissance, expressed mainly in the pneumatic revolution in chemistry and in the emergence of electricity— was also a product of the eighteenth century. The third wave, associated with the atom and the quantum, had to wait till the twentieth century. The task of the nineteenth century in the physical sciences was primarily to extend the earlier discoveries and to coordinate them into more far-reaching system. The expansion of science marched hand in hand with that of industry. The laws of the conservation of energy and of the electromagnetic field are its greatest achievements. It was rather in biology, with Darwin's theory of evolution, that a radical reformation was to be effected. Actually even in this field, as well as in those of physics and chemistry, the new advance was in harmony with the individualism, competition, and progressiveness of the times. Darwin's nature was a passable imitation of the ruthlessness of industrial capitalism.

Here, however, we are concerned mainly with the physical sciences, and have to explore and bring out, both in their general lines and in particular detail, some of the forms of interaction between science and industrial progress. The problem can be tackled from either end. We might take the development of industry and trace the degree to which each radically new step was inspired by science, and how in turn the experience of industrial processes set problems which led to scientific advance. Alternatively, we might start with the history of science, and enquire where the source of inspiration could be traced to some economical or technical source and where a scientific discovery was to find application in industry. The former approach is more suitable for the beginning of the century where science, for all its internal perfection, only came incidentally into industry and had not yet touched its basic processes, such as coal mining, ironworking, weaving, or spinning. The latter becomes almost inevitable towards the end where whole industries like the electric light and power, or the chemical industry, were being built largely *de novo* on scientific discovery. The best compromise I have been able to make in the modest range of this essay is to start with a general survey of industrial trends in their relation to science; then, more briefly, to introduce some of the major scientific themes of the century, bringing out the degree of their industrial inspiration or use, and finally to try to link the two together and arrive at some general conclusions on the nature of the interaction[1].

The characteristic difference between industry in the eighteenth and in the nineteenth century lay in quantity rather than quality. The basic

innovations, those of power-driven machinery replacing handwork and of steam rather than water as a source of power, had already been made before the nineteenth century opened. The great difference was that they were now being used on an ever-increasing scale and with corresponding increases in cheapness and efficiency. The necessary social concomitant to the large-scale use of machinery—a plentiful supply of property-less labourers and easily obtainable capital—had also begun in the eighteenth century, but its rapid extension on a large scale in the nineteenth was a new social phenomenon. It brought about first in Britain, and later over most of Europe and North Eastern America, the capitalist method of production with its class system and the replacement of feudal by economic dependence. In fact it was the quantitative change of scale itself that gave its characteristic quality to nineteenth century industry and economy.

The dominant industry of the nineteenth century was the same textile industry which had given rise to the industrial revolution. The production of cotton cloth had increased manyfold from 40 million yards in 1785 to 6,534 million yards in 1887, but the principles of working and the machinery remained substantially unchanged. This phenomenal growth could, however, only take place by a general transformation of all industry to provide the plant, materials, power, and labour for the textile industry and for the distribution of its products. This meant a prodigious development of the old coal and metal industries and the virtual creation of a new one—the machine building or engineering industry. All these, as well as the textile industry itself, required power, and now power was steam power dependent on coal.

It was in the first place the need for the transport of coal that gave rise to the characteristic new technical contribution of the nineteenth century—the railway. The railway was a consequence of the development of mining, where rails had been in use at least as far back as the fifteenth century. It was the provision of iron rails and the attachment of a steam engine on wheels—the locomotive—that took it out of the mines, and made it for nearly a hundred years the universal carrier on land. Nevertheless despite railway and steamship, the cost of transport still tied industry largely to the coal fields. The steam engine and the factory chimney remained all through the century the symbols of the dirty, formless, gas-lit cities of the first industrial age. The very convenience of mechanical transport served in the first place to enable these concentrations of population to be supplied with food and raw materials.

The rise of mechanical transport and communication, and all they

entailed, is the key to nineteenth century progress. What the railway did inside the new industrial countries, the river steamboat and later the ocean steamer did in the rest of the world. It was thrown open to the sale of cheap industrial products, which ruined native industry, and to the exploitation of colonial raw materials, of which cotton came first and foremost. Only towards the end of the century did the export of capital, in the form of rails and mining machinery, and the import of food for industrial populations, no longer self-supporting, herald the era of imperialism and monopoly.

Neither the textile and coal industries, nor the railways and shipping as such, depended on science or contributed much directly to its advance. What effect they had was, as we shall see, indirect, through ancillary developments—in chemistry through bleach and dyes; in physics through the telegraph; and in engineering and geology through the large-scale work on cuttings, tunnels, and bridges. An infinitesimal proportion of industrial, transport and trade profits went to help science, but they were absolutely so substantial that what they did give provided more for science than the benefactors of all earlier ages.

Much more directly important, however, was the influence of the great prime mover of the nineteenth century—the steam engine. The eighteenth century had left a great legacy in an engine that worked well enough to be able to drive machinery, but its low efficiency limited its use far from the coalfields, and its clumsiness and small power output per unit weight for a long time held back its application to railway traction. Here was a challenge to generations of scientifically minded engineers and physicists with a taste for mechanics. How did the engine work? How was heat really transformed into power? The young genius Carnot, working in France, where the steam engine was foreign and could be looked at objectively, had found the answer by 1830. However half of it—the conservation of energy (the first law of thermodynamics)—was lost on account of his early death, and the other half—the principle of availability of energy (the second law of thermodynamics)—was hardly noticed for a quarter of a century, until in 1848 it was taken up by Clausius and Kelvin.

The great delay might appear as the accidental product of a personal tragedy, but in fact much of the pressure behind the enquiry had been blown off in the interval by a self-educated, almost uneducated, son of a colliery engine man, George Stephenson. In 1829 he hit on the common-sense solution of using the exhaust steam to force the furnace draught of

his locomotive, which increased its speed from four to thirty miles an hour and made it a practical proposition outside the mining districts. The railway age was on, and science could take a back seat.

Meanwhile Mayer, a German doctor, and Joule, an amateur with a mechanical bent and a flair for measurement, had independently rediscovered the principle of the conservation of energy and measured the rate at which work could be converted into heat. It remained for Kelvin, Helmholtz, Clausius, and Gibbs to build these results into the unified science of thermodynamics. It is impossible here to pursue more closely the history of this great generalisation. Elsewhere[2] I have attempted a detailed analysis. My own studies leave me in no doubt as to the crucial importance of the practical development of prime movers in general, and of the locomotive steam engine in particular, for the whole foundation of thermodynamics.

The conversion of work into heat, known from the highest antiquity—stone age man lit fires by friction—had raised only the most general curiosity. This was not surprising—there was no money in it. Rumford had even shown in his experiment on boring cannon that the method had no future as a source of heat. More heat could be got by burning the hay then by feeding it to the horses that turned the gin. But the converse, the turning of heat to work was a different story. Watt and Boulton had made their fortunes by guaranteeing a greater "duty" in water lifted by their engines per ton of coal burnt, than by older methods. In the process they introduced to science the concepts of work and horse power foreign to Newtonian dynamics. What difference was there between the actual return and what might be obtained from a more perfect engine? This was the question that led Carnot to introduce the key notion of a reversible cycle, one from which the last ounce of available energy could be squeezed.

From Carnot onwards the steam engine was not so much in evidence in the theory of heat. It was no longer needed explicitly and could be replaced by progressively more abstract mathematical symbols. But this does not diminish its essential importance in first posing the problem of the interconvertibility of heat and work and later of all forms of energy. Indeed this demonstration of convertibility did much to give scientific sanction to the general idea, very acceptable in the free-trade era of the mid-nineteenth century where money really counted, that all the forces of nature could be valued by a common measure.

Rapid communication was a requirement which grew with the growth

of rapid transport. Not only was it necessary to operate the railways and direct the steamers, but the growth of business that went with the growth of trade put a premium on the rapid availability of news from distant parts. What with the stimulation this gave to inventors and the means which the newly discovered electric current offered, telegraph systems were invented and reinvented in many countries[3]. They underwent a rapid development in rapidity of working, leading to the understanding of the use of more and more complex circuits.

The telephone was a natural improvement on the telegraph but took forty years longer to perfect. This was largely because it involved bringing in a scientific analysis of sound and hearing. It is characteristic that Graham Bell, who first patented a practical telephone in 1876, was a teacher of elocution who was much influenced by Helmholtz's theories of the physiology of hearing. Once established, the telephone was able to inherit, so to speak, all the equipment and experience of the telegraph lines, batteries, switch gear, etc. and consequently to develop much faster. It was further, as primarily a domestic appliance rather than one confined to post offices and stations, to pave the way for distribution systems of electric light and power. The electric telegraph and later the telephone caught the public imagination of the nineteenth century as being real wonders of science, much more difficult to grasp intuitively than the workings of the steam engine.

The early development of the telegraph was to have an enormous direct and indirect effect on the development of physics. It provided a new scope for it in the teaching of telegraphists. Further, the development of apparatus for the operation of the land telegraph, and even more of the undersea cable posed many fundamental problems in electricity and stimulated a greater interest in it. The greatest minds in physics did not scorn to make practical contributions to telegraphy. Of particular importance was the long struggle (1857–66) of William Thomson—made Lord Kelvin for his pains—to get the Atlantic cable to work. In the process he not only devised new instruments, but by his appreciation of the capacity of the cable took the first step towards an understanding of oscillating circuits. The development of telegraphy supplied at the same time equipment and apparatus—switches, insulated wires, and galvanometers—which before its advent the experimenter had had to make for himself.

The impetus to exact measurement and the setting-up of standard units in magnetism and electricity came largely from the needs of reliable operation of batteries and circuits. Great names in physics—Ohm, Gauss,

Weber—turned electricity from a fascinating qualitative study full of unexpected effects into a precise quantitative discipline. In doing this they made real electrical engineering possible. Practice was not the only gainer; with reliable units new quantitative laws began to appear. It was Maxwell's identification of the ratio of electrostatic and electromagnetic units with the velocity of light that clinched his electromagnetic theory of light and later made wireless possible. No more need be said at this stage about the developments of electric light and power in the latter part of the century because that belongs to the second part of the story, that of the birth of new heavy industries based on originally purely scientific premises.

The demands of the older textile industries and of the newer industries, engineering and transport, that had grown up to serve them, led in turn to an enormous increase in the demand for metal. By mid-century cast and wrought iron were largely replacing wood as a material for machinery of all kinds and even becoming important building materials. The basic invention of making iron with coal had been made in the eighteenth century. The first half of the nineteenth century was to see the enormous extension and the steady improvement of the iron industry without any radical change. Science, however, was to enter the field of metallurgy relatively late but with decisive results. Bessemer first made cheap steel in 1860 by a logical application of chemistry. The revolution he started was completed in 1879 by Gilchrist Thomas whose basic process made available for steel-making the abundant phosphoric ores. Cheap steel was to be as important in the 19th century as cheap iron in the 18th. Applied to railways, ships, and buildings, it was to be a major factor in the rise of heavy industry.

As might be expected from its highly traditional nature, the metal industry, particularly its largest branch the iron and steel industry, throughout most of the century drew very little on science and contributed even less to it. It was eminently the field for the practical man with know-how at the bottom, and the ruthless captain of industry at the top. But towards the end of the century this situation changed for good. Bessemer, though scientifically-minded, could hardly be called a scientist, but Sir. W. Siemens and Gilchrist Thomas definitely were[4]. It was their success in founding the new industry of steel that was to give an impetus to a really scientific metallurgy, beginning with Sorby's use of the microscope in 1864. It was not, however, until well into the twentieth century that an all-round scientific study of metals could begin. Partly this was on account of the intrinsic complexity of the metal state which required quantum theory

and crystal physics to unravel it, and partly because of all heavy industries, the metal industry depended most on the manual tradition of the smith turned ironmaster.

The metal industry evolved throughout the nineteenth century as the provider of raw materials for a new, machine-building, engineering industry with which, from the days of Wilkinson and of Boulton and Watt, it was closely linked. The engineering industry grew up as a blend of that of the crafts of the clock and instrument maker on one side, and of the smith and mill-wright on the other. At first it had to create its own technicians and its own workers at the same time as it produced the new machines. It was only by the twenties of the century that men like George Stephenson, born and bred with engines, began to appear. The twin aims of the industry were precision and power. The succession of great technicians, all starting as manual workmen—Bramah, Maudsley, Whitworth—created the machine tool and led to the making of accurate and standardised parts. It represented an extension of mathematics into large scale metal working to achieve an accuracy and reproducibility impossible with wood. On the other side of the Atlantic, the elder Brunel, Eli Whitney and Colt used the results of accurate manufacture for the first assembly of interchangeable parts. This was first, characteristically enough, for light weapons and then for the great labour saving mechanisms of the sewing machine, the typewriter, and the reaper and binder.

Where forces much greater than man could wield were needed, Bramah's invention of the hydraulic press and Naesmith's of the steam hammer made heavy engineering possible. Towards the end of the century, steel making and engineering in Britain, America, Germany and France were already concentrated in large firms, the prototypes of the trusts and cartels of the twentieth century. They were becoming more and more concerned with the business of imperial expansion and with the increasingly expensive and profitable manufacture of the guns and battle ships needed to secure the fruits of empire.

The engineering industry, though more linked with science than the making of metal, still remained largely outside the main scientific movement of the nineteenth century. This was largely because, for most of the time, the problems raised in mechanical engineering, where they were mathematical, as in the design of moving parts, were soluble with the relatively simple geometry and calculus of the eighteenth century, and where they were physical, as in friction and fatigue, were beyond the resources of nineteenth century physics. Indeed as the century wore on

the close relation, almost the identity, between the scientist and engineer of earlier times, exemplified in such men as Smeaton and Watt, gave way to a new type of professional engineer. The great quantity of nineteenth century machine production led to a temporary decline in the scientific quality of engineering. The nineteenth century engineer no longer relied exclusively on craftsmanship and tradition, but he depended far more on formulae and tables than on original research for solving his problems. This was in part a necessary consequence of laisser-faire economics. The problems had become too difficult and extensive to be within the reach of the individual or small firm, while co-operative and state-aided research had not yet come in.

It was only towards the end of the century, with the growth of big semi-monopoly firms, that the radically new development of the turbine[5] was evolved to meet the needs at the same time of naval power and electric generation. It was to point the way to the new triumphs of aerodynamics of the twentieth century and to lead to a new integration of science and technique.

So far we have considered mainly the relations of science to the development of the old traditional industries but the third quarter of the 19th century was to witness the rise of one industry, the heavy electrical industry, which was of purely scientific origin. Although this industry drew much from that of the telegraph and cable industries it was not any true sense a continuation of them. The earlier electrical industry dealt only with feeble currents and used apparatus that could be carried in the hand or mounted on tables, the newer was to compete in power and weight with the steam engine and the rolling mill. In relation to its earlier phase it was as the mill to the watch, both with the same mechanical movements, but with radically different purposes, the one to alter matter, the other to convey information.

The history of the development of the heavy electrical industry turns round the two poles of the generation and utilisation of electrical current. It was Faraday in 1831 who first showed how to convert mechanical power into current, but it was not till fifty years later that this knowledge was effectively used on a large scale. It is one of the major problems of the relations between science and technique to explain the reason for this delay, occurring as it did in the very midst of a bustling century. The failure to utilise Faraday's discovery has been variously put down to the purity and unworldliness of the scientists and to the great technical difficulties of turning a laboratory experiment into a practical working

machine. Both these factors certainly operated. With the exception of a few far-sighted scientists like Jacobi in St. Petersburg and Joule in Manchester, few realised the practical potentialities of the electric current as a means of transmitting not merely messages, but *power*, and no individual or corporal body even considered the problem as a whole.

It was also true that a number of technical snags existed in the design of dynamos and motors—in the proper disposition of the coil and field magnets—and in the operation of the machine, brushing, insulation, overheating, etc. But none of these separately or altogether would have taken more than a dozen years to get over if there had been any such concentrated attack on the problems as was given to the steam engine in the late eighteenth century or was to be given to electricity in the decades after 1880. The real historical problem is precisely to find out why so little scientific engineering effort was put into the generation of electricity between 1831 and 1881.

The answer turns out to be far more economic and social than technical. In a century as commercial as the 19th, the problem was not really how to make electric current, nor even how to use it, but rather how to sell it. A vicious circle had to be broken. Unless there was a market for electric current it would pay no one to produce cheap means of generating it, and until cheap means of generating it existed, its price would be so high that it could command no market. The ideal market in the nineteenth century was one in which a commodity could be sold in large numbers at a high profit. This was found in the electric light. The idea of selling energy, transferable only for short distances, was a new one but was assimilable to that of the distribution of normal fluids like water and gas. All depended for their profitability on the existence of large urban communities which nineteenth century industry and commerce had called into existence.

Electricity could only begin to be used for power if current generated for another purpose could be produced and delivered where wanted. Although for its other main use, traction, electricity could be separately generated, this also depended for its profitability on the existence of large cities. Nevertheless the London, Paris, or even the New York of the fifties and sixties were already quite large enough to absorb electric light so that this condition, though necessary, cannot have been sufficient.

The real hold-up occurred because in the nineteenth century there was no means of financing research and development, based though it might be on established scientific facts, even to meet a known need. The advance had to proceed stage by stage, the profits on one paying for the research

on the next. I have shown elsewhere how, in the development of current generation, the demand was first in the forties for hand-operated electrical lecture demonstration apparatus, then in the fifties for electro-plating. It was only in the sixties that steam-driven machines came in with the demand for arc lighting of lighthouses, depots, streets, and exhibitions. It was this that turned the corner in establishing a large-scale demand for electricity and in directing its application into the channel of consumption that was to be yet more profitable than that of electric arc light. The demand was already sufficient to justify economically the major developments of the shuttle armature by Werner von Siemens (1860), regenerative working by Wilde (1866), and the Gramme ring armature (1870). These together made the modern dynamo and its almost identical team mate—the electric motor. If to these we add the alternating current generator, which owes its modern form also to Wilde (1868), and the transformer by Gaulard and Gibbs in 1881, the complete range of power generation, transmission, and utilisation had been reached. Indeed no basic changes have occurred since that time.

There was still one major problem to be solved—what was called in its time "the dividing of the electric light". This really fell into two parts—the provision of a small, safe, durable and low current source of light usable in houses, and the arrangement of the generator and current circuits to run them. This was to be the work of Swan and Edison in the early eighties. Swan was a photographic chemist with a flair for careful experiments and a basic knowledge of the properties of the cellulose from which the first carbon filaments were made. Edison was a self-trained telegraphist of enormous enterprise and inventive capacity. His experience with wiring up of ticker machines gave him all the ideas he needed for power station and service networks.

In this development of a public lighting service the three strands of telegraphy, generator design, and vacuum bulb technique first fused together and effectively created the heavy electrical industry. Its growth from then on was secure and rapid. In a decade electric light changed from a luxury to a necessity, while electric transportation became important in the nineties. Nevertheless, the real impact of the electrical industry as the intermediate prime mover of all mechanical production was not really felt until well into the twentieth century.

The impact of all these developments was profound. In the first place the mere establishment of a major industry entirely based on science and dependent on it, not only for its improvement but for its current operation,

gave science, and particularly physics, a solid and permanent base. Electricity had become part of everyday life, and what is more there was big money in it. With the profession of electrical engineer offering an ever-increasing number of jobs, there was bound to be a growing demand for teaching and research. Indeed, it was in connection with electrical engineering that physical research left the bounds of the universities and was established in industry itself. Edison's Menlo Park laboratory may have been a crude affair, but it was the prototype of the great governmental and industrial research institutions of today. It is in the electrical industry that we can see most clearly how economic success can, so to speak, *fix* and establish scientific *development*.

The purely scientific effect of the advent of electric light and power was also enormous. The behaviour of dynamos, motors, and transformers played a similar role in promoting electromagnetic theory and the knowledge of magnetic materials, electrical conductors and insulators, to that of the performance of the steam engine in the development of the theories of heat. There was, however, a significant difference; in the earlier case science followed well behind practice, in the later it had, in principle, the lead all the time. Here we can see the transformation from an industry making use of science to one dependent entirely on it. More profound and radically new developments in science were to follow the commercial production of electric bulbs. Vacuum technique and a supply of cheap components revolutionised laboratory technique. This opened the whole new field of electronics that was to transform not only science, but communications and industry in the twentieth century.

It should be clear from this example alone that it is impossible to treat separately the development of technology and science in the nineteenth century, still less in our time. From the moment that the telegraph became a practical reality all the new conceptions of electricity—resistance, impedance, self and mutual induction, hysteresis, together with the units used in measuring them—were a blend of scientific theory and experiment, technical experience and commercial exploitation. The same men busied themselves in many of these aspects together.

Further, the whole network of derivation of ideas from observations and the realisations of these ideas in new applications, which make up the history of electricity, cannot be unravelled without destroying its significance and reducing it to a mere list without logical connection. Although the main framework of the science, dependent as it was on objective facts, could not be altered, social and economic factors were everywhere at

work, slowing down one stage of its discovery, hastening another. Even the general pattern of the science was determined by the order in which the discoveries in its different branches were made and consequently by these social factors. Nor was the encouragement, or lack of encouragement, that was provided through commercial or government circles the only way that society determined the rate of advance. Quite as much, as we shall see, was effected through the leading ideas of physics that were part and parcel of the whole way of thought of the time.

The second great scientific industry—the chemical industry—was never so entirely dependent on science as was that of electricity. It had a long ancestry going back to the cooking, tanning, and pottery of primitive man, but it had, in the last quarter of the 19th century, got well away from rule-of-thumb and old recipes. By that time chemical science had become complete and autonomous enough for the industry to rely on it largely for its day-to-day operation and entirely for any future development.

Unlike the electrical industry, which turned on the successive discovery and utilisation of a few mathematically linked principles, the chemical industry showed a far more empirical and multifarious relation between chemistry and practice. All through the century the relation of the chemist to industry was far more immediate and intimate than that of the physicist. In histories of chemistry, unlike histories of physics, it is impossible to leave out or even minimize the connection between the two. Further, it was generally recognised even at that time that the industry helped chemistry as much as the other way round.

Indirectly, too, chemical industry was linked to the growth of science by the increasing demand for trained chemists, and consequently to the fortunes of teaching laboratories and to facilities for research. This was on a far greater scale than for the other sciences. It is probable that all through the century more than half the scientific men in the country were chemists, though it must be remembered that this was not an exclusive profession in the early days. Dalton was almost as much a physicist as a chemist, and Faraday almost as much a chemist as a physicist.

At the beginning of the century the new chemistry improved, accelerated or short circuited old traditional processes; by the middle it was imitating natural products artificially and creating, from cheap materials, new ones of the same kind; by the end it had become an industry in its own right getting ready to undertake, in our own time, the large scale synthesis, from the elements, of substances of unheard of properties. Yet all through the century the chemical industry remained a relatively small ancillary to

the dominant industry of the time—that of textiles. The heavy chemical industry was mainly concerned with producing enough processing materials—soap, acid, alkali, bleach—for the ever expanding needs of the cotton and wool trade. The fine chemical industry was made by the first discoveries of synthetic dyes for the textile industry, though new drugs began to appear as a by-product. Even the origin of the new synthetic chemical industry—that of plastics—began with the treatment of textile fibres, first with alkali in mercerisation and then with acid leading to celluloid and gun cotton. This, coupled with nitroglycerine and its embodiment in dynamite, was to be the beginning of a new explosives industry, which in the next century was to link the new chemical monopolies closely to the service of war.

The relations between chemical industry and science were made easier because, for most of the nineteenth century, the chemical industry was composed of a large number of small independent units. The only approach to large works, and that towards the end of the century, was in those for soda, sulphuric acid, and soap. There was scope for small men, usually druggists, to set up the manufacture of some new line, either in the improved purification of some natural product or the production of some new and cheaper substitute. Chemical discoveries could soon be turned to use. Scheele's methods of separating vegetable acids led to the setting up of a number of small firms making citric and tartaric acids from waste fruit juices. Chevreul's work on the natural fats led to the use of stearic acid instead of tallow for candles. Later Dumas, from an investigation of the stink given off by stearic candles bleached by chlorine, discovered the law of chemical subsitution. In another way, chemists frequenting works, or manufacturers who learned some chemistry, had opportunities for noticing odd reactions or curious by-products of scientific interest and commercial value. Thus Courtois in 1812 discovered iodine as a by-product of the burning of kelp for soda, and Berzelius in 1817 discovered selenium in the chimneys of a sulphuric acid factory.

Two independent sections of the chemical industry, though not considered to be so till fairly late, were the drink and food trades. Because of their continuous and most ancient traditions they were at first resistant to the intrusion of science. When they changed, however, from a household or village basis to the scale necessary to satisfy the needs of the new industrial towns, new problems arose which tradition found it hard to get an answer. At first it was science that had to learn from practice, but soon it returned the gift. The first contact was made with physics because it was simpler.

The behaviour of whisky stills[6] had given Black the idea of the latent heat of vapours and through him Watt hit on the separate condenser which revolutionised the steam engine. It was Mr. Thrale, Samuel Johnson's friend, who first used a thermometer in brewing. Later, when chemistry had advanced far enough to be able to give some practical aid, chemists were called in to cope with the all too frequent cases where the change to large-scale and rapid operation led to unpleasant and unprofitable results.

The key problem here was the scientific understanding of the apparently spontaneous changes which occur in vegetable and animal products, coming under the general term of *fermentation*, which had been used by man from the earliest times. This problem is not yet solved, but from attempts to solve it were to come new methods of preparation and of preservation, which transformed the drink and food industries and made them more or less adequate to the needs of an expanding industrial population cut off from access to fresh natural products. Even more important to the human race, it led to an understanding of infectious disease which was ultimately to be the basis of the first scientific and really effective medicine, preventive even more than curative.

I have discussed elsewhere in some detail the way in which the incentives provided by the fermentation industries, and even more the scientific problems they posed, led step by step first to the understanding of the chemical processes that occurred in fermentation, then to the role of the living agents—the yeasts and bacteria—in bringing them about and lastly to the nature of the active chemical substances, the enzymes, that were the immediate agents of the change. In the process the new knowledge was to strike roots in pharmacology, in the preparation of vaccines, and in agriculture, in the utilisation of the chemical principles of fertilisers, and in dealing with plagues and pests.

In the story of the life work of such figures as Liebig and Pasteur the interplay of scientific and economic interests is evident at every turn. Both aspects occur repeatedly, the discovery of some new scientific fact from the observation of an industrial process and the practical application of the results of experiment and theory. It should be apparent that both Liebig and Pasteur succeeded because both, in their different ways, felt strongly that it was not only necessary to advance knowledge but also to see that scientific advances were widely known and profitably used. They were both disinterested in the sense that they sought no personal profit, but not at all disinterested in securing the greatest social effect for their

work. In this, both were anticipating the driving tendency of twentieth century science. Both began to see their reward in their own time. Liebig witnessed the beginning of scientific agriculture with the use of fertiliser and of a rational large scale food industry. Pasteur had wider rewards commensurate with the range of his interests.

The germ theory of fermentation and disease was already in the nineteenth century creating new possibilities of securing food supply and opening up new territories for exploitation. It gave a rational basis for food preservation, sanitation and the control of epidemics. Together with the work of Claude Bernard on chemical physiology, it turned medicine from a venerable tradition into an applicable science. Now the doctor could, for the first time in history, intervene with understanding to help the curative processes of nature and sometimes succeed where nature alone was bound to fail.

The effect of Liebig and Pasteur on science was hardly less great. Liebig, as a research worker and even more as a teacher, was one of the main founders of organic chemistry. Pasteur's first work on molecular symmetry provided the key to the spatial representation of atoms in combination, and is the basis of modern structural chemistry. The great controversy about the living or non-living character of ferments has proved a most fruitful one. Both the protagonists have been proved, in the light of subsequent knowledge, to have been right as well as wrong. Pasteur's microbes turned out to be the essential agents, not only for the fermentations but for Liebig's chemical cycle, the nitrogen cycle in the soil where the humus was restored to its important function. On the other hand Buchner showed, after Pasteur's death, that ferments exist as chemical entities (enzymes) and that the living organism is not essential for their action but only for their formation. Modern biology is tending to rest, more and more, on the basis of chemical reaction directed by organically formed molecules, the joint work of Liebig and Pasteur.

These considerations should show something of the effect of industrial development on the advance of science. The major topics of investigation were accordingly limited by the essential character of nineteenth century technology. Taken in all, this was not yet a technology consciously aiming at achieving new results, but rather at satisfying old needs in new ways—it was concerned with means and not ends, apart from the universal end of money making. A few new requirements were created, but the old requirements for food and clothing were satisfied on a larger scale than ever before, though the advantage to the individual was largely eaten up

by an unjust and wasteful social system. The new goods involved less labour, but the ingenuity that had been devoted to their production had been concentrated on cheapness rather than quality or serviceability—they had been produced for profit rather than use.

Production for profit, and the utilisation of part of that profit for further capital investment, was indeed the great motive power that had brought the industrial revolution into being. It was still urging it, in the latter part of the nineteenth century, when the early successes of the new industry were making capital harder and harder to invest profitably, except in colonial territories or for the wasteful uses of war. The era of classical, competitive free-trade capitalism was passing, though the consequences of the use of the productive machine for profit were only to be seen in the next century. The characteristic of this chase for profit was its blindness—a blindness not concealed but actually praised by the dominant advocates of laisser-faire. Free competition was the watchword throughout most of the century, though as it wore on both technical advance, requiring more expensive capital equipment, and financial interest, in building price-fixing rings and cartels, were more powerful real forces, which were to become dominant by the end of the century.

I have traced in barest outline some of the more important ways in which the direction of scientific effort was guided, often consciously though rarely in an orderly way, along channels which arose from technological and economic needs. The same story might have been told in a different order if I had started to trace the main lines of advance of science, rather than those of technology. Even so the major emphasis would have come in the same places.

In physics the outstanding achievements were the establishment of thermodynamics—the conservation and transformation of energy—and of electromagnetism—the coherent theory uniting magnetism, electricity and light. The first of these, as we have seen, arose rather belatedly from the economics of the steam engine. The second had, it is true, in the beginning an almost purely dilettante and philosophic character, but was carried triumphantly forward when it achieved its first paying contribution in the telegraph. The history of thermodynamics and electricity, indeed, bear witness to the power of the seventeenth century mathematical analysis which we associate with Galileo and Newton, but it is worth remarking that in both cases this failed to cope with the facts till they had been reduced to intuitive mechanical conceptions by such practical experimenters as Watt and Faraday. Indeed it might well be argued that in

their early stages mathematics proved more of a hindrance than a help to heat and electricity.

The importance of industry, agriculture and medicine to the advance of chemistry in the nineteenth century is beyond dispute. There the interaction was continuous; the leading ideas of substitution, radicles, valency, and stereochemistry all arose immediately out of problems arising from practice. Already in the eighteenth century the old mystical neo-platonist alchemical ideas had been blown away, but Newtonian physics was as yet in no position to offer a convincing alternative. All through the nineteenth century chemistry was still in the stage of turning from a purely empirical science to one of increasingly systematic description. It was not indeed till the twentieth century that any serious mathematics, outside thermodynamics, entered into chemistry at all.

Taken in all, the achievements of the physical sciences in the nineteenth century represent an enormous extension of the comprehensibility and coherence of the world of matter and force. If it did not look very deep into the structure of that matter or the nature of the physical forces, it did show how they were all—heat, light, sound, electricity, and magnetism—related to each other, and how they were linked to the behaviour of matter according to the laws of thermochemistry, electrochemistry, and spectroscopy. Over the whole field the emphasis was on fact and law.

What had to be done was to establish the relations and measure the properties of matter in the appropriate units. The nineteenth century scientists did, in fact, largely succeed in completing Newton's original programme as expressed in the preface to the *Principia:* "The whole burden of philosophy seems to consist of this—from the phenomena of motions to investigate the forces of nature, and then from those forces to demonstrate the other phenomena", but with his own rejection of the occult or hidden forces that were to be so prominent in the twentieth century. Because of its factualness, because it all sounded like the inventory of a well-ordered general store, the physical sciences came to have by the end of the nineteenth century an oppressive dullness and finality. There was a feeling that knowledge might be improved, but that what was unknown was merely more of the kind of thing one knew already[7].

This limitation, which was so soon to be swept away, did not in any serious way effect the practical applicability of the physical sciences. Indeed as the century progressed more and more aspects of industry could be handled quantitatively by scientific methods and, as we have seen, science was beginning to generate radically new industries. In doing so,

science itself entered as a permanent, positive and indispensable element in the productive process. This was a major transformation whose consequences have yet to be fully appreciated.

Compared with previous centuries, the nineteenth witnessed an enormously rapid advance in science. Though lacking either the great technical or the great scientific originality of the latter eighteenth century, it was on an altogether larger scale; where previously scientists and engineers could be counted in tens they could now be counted in hundreds and thousands.

Compared, however, to the twentieth century which inherited its achievements, the nineteenth century advance was spasmodic, uneven and inadequately sustained. Where discoveries opened new fields, as in the case of electromagnetism and organic chemistry, there were long delays in following them up, only ended by an overwhelming case for profitable exploitation. This is in keeping with the general character of a rapidly growing competitive capitalism. This growth, with its ever increasing markets opened up by rail and steamship, put a premium on the multiplication of existing production, rather than on technical advance. The key industry—textiles—showed no fundamental improvement throughout the whole century. Where the very quantitative increase led to new requirements such as those for rapid communication, there was a demand for radically new solutions. Even there, however, the opportunities for introducing technical changes were limited. In boom times, markets were assured without any need for innovation; in slumps new investment was unthinkable. Only in periods of recovery was it worth while investing in them. The capitalists were willing to accept an obvious innovation of proved workability when it was offered from outside, but rarely to put up money of their own for its invention. This is well brought out in the case of the development of steel, where not one of the major improvements came from inside the industry itself.

The attitude of the scientists in this highly individualistic, not to say anarchic, period cannot be comprised in any simple formula. Indeed different men, and even the same men at different stages of their careers, were repelled from, and drawn into, cooperation with industry. The nineteenth century witnessed at the same time the establishment of the ideal of pure science and the first industrial research laboratory.

The majority of scientists were not eager to intervene in industry. Throughout the century there was a growing separation of the scientists from the manufacturers. The intimate, personal and family connections that had existed between science and industry in the later eighteenth

century gradually diminished as the new century wore on, and found their echo only in the annual beanfeasts of the British Association. This separation was quite as much due to the success of science as to that of industry. As the century progressed science began to play a larger and larger part in the universities and government teaching establishments, first in France, then in Germany and Britain, last in Russia and the United States, to name only major countries. The talented and often self-educated amateur who in the earlier part of the century practically monopolised science, except for a tiny elite of academicians, gave way to the university-trained professor, and by the end of the century could no longer compete in scientific discovery.

It was otherwise, as we have seen, in industrial advance. Even at the end of the century the major innovations were still coming from a race of inventors without a university background, who had learned their science from books and experience in laboratories made by their own hands. The great success of inventors like Edison was, however, to presage a new phase what might be called the industrialisation of invention, with the setting up of large research laboratories. The industrial laboratory, and the government research laboratory that came with it, brought science back into industry in a new way. The consulting scientist and the scientific entrepreneur were gradually replaced by the whole-time salaried scientist, and the new profession of scientific research worker was created. These changes, though they were brought to completion by the 20th century, were visible as tendencies towards greater organisation and a recognised status for science and technology, all through the century.

They brought with them advantages and disadvantages. At the beginning of the century the absence of any provision for training and finance was compensated by the relatively simple nature of applicable science, and the opportunities for the amateur and the inventor to be accepted into the ranks of science and industry. Before the century was over the facilities for learning science in highly industrialised countries, particularly in the newly industrialised countries, such as Germany, were becoming comparable to those for the older learned professions, while, at least in Germany, scientists were winning a recognised place in the direction of industry. On account of those very reasons the entry to science became more and more limited to a intelligentsia drawn largely from the minor bourgeoisie, and it became almost impossible for the lone inventor to succeed unless he came to terms, usually rather unfavourable terms, with the big firms. There were to be no more Gilchrist Thomases.

For these reasons many scientists today, particularly in the older universities, look back with regret to the 19th century ideal of free science, usually without considering its limitations. They look back in vain, for it is as far beyond recall as the Middle Ages. The general development both of science and industry lead inevitably to higher forms of organisation; our task is not to resist this, but to see that what organisation is necessary is used effectively and for good ends.

Throughout the 19th century the scientist had enough to do to establish himself as an acceptable member of an old and tradition-ridden academic society, where he tended to imitate his colleagues in other faculties and draw aside from the industrial world, partly from intellectual snobbery, partly from a more worthy disgust at the unashamed money-hunting and philistinism of the business man. Out of this compound was born the ideal of pure science, the ethic of what was to be a great new profession comparable with the age-old professions of law and medicine, and like them primarily devoted to the service of the upper classes. Indeed, the withdrawal of the scientists from contact with industrial realities was derived from the existence of a profound contradition at the root of 19th century society. It was difficult, indeed almost impossible, to satisfy the absolute need for science to draw its inspiration from practical human enterprise without engaging in, or at least conniving with, the universal corruption of industrial effort to unworthy ends.

The contradiction is indeed an unescapeable one under the conditions of capitalism, and finds its resolution only in a change to a social system where the scientists can work practically with and for the whole people. Throughout the 19th century it only served to present the scientist with increasingly painful dilemmas. The choices they made are revealed in the study of their lives and actions. Only a few great scientists like Carnot, Liebig, Pasteur and Kelvin managed to contribute directly to the economic progress of their time. Most of these, for one reason or another, themselves escaped the corrupting influence of wealth, but all contributed indirectly to the wealth of those capitalists who exploited their ideas. Others, like the Siemens and Nobel, threw themselves wholly into the creation of new scientific monopoly industries. Both groups were, however, exceptional. The main body of scientists worked, as the century drew on, in an increasing divorce from the great industrial developments of their time.

The most farseeing of these scientists were, nevertheless, contributing their share indirectly in the essential work of systematising and rationalising pioneer observation and experiments and thus providing a basis for new

advances. Clausius and Gibbs followed Carnot and Mayer and helped to found a new chemical industry. Maxwell and Hertz followed Oersted and Faraday giving rise to radio and to all of modern electron physics. In spite of this it must be admitted that much of the intellectual effort going into academic science in the nineteenth century wasted itself on sterile exercises on outworn themes. A perusal of old numbers of scientific journals makes this too deadly clear. Even the one justification it offered, its contribution to the teaching of new generations of scientists, was often nullified by the dryness and dogmatism of its teaching[8].

It is not altogether surprising that practical business men had no use for science of this sort, but they offered little enough encouragement to develop it in directions more useful to them. Indeed what so hampered and slowed down the technical development in the nineteenth century was the complete lack of any systematic way of financing research and development. The scientists had to depend on the luck of an interested patron, or, even more often, had to finance one development out of the success of another, where one failure could stop research full of promise. The infantile mortality of invention, so to speak, must have been very great in the nineteenth century.

It would be misleading, however, to give the impression that there were no scientists who tried consciously to bring science in an organised way into the field of practical progress. This was peculiarly the task of the British Association for the Advancement of Science, a most characteristic 19th century creation. The movement that gave rise to the British Association was, itself, the result of the awakening liberal movement in Germany. Its parent body was the Society of German Naturalists and Natural Philosophers, founded in 1822 by that fantastic, but courageous pioneer of *a priori* 'Naturphilosophie', Oken. It was Babbage, conscious of the "Decline of Science in England"[9] and in particular the failure of the Royal Society to live up to its seventeenth century aim "to improve the knowledge of naturall things, and all useful Arts, Manufactures, Mechanick practises, Engynes and Inventions by Experiments ..."[10], who was inspired by the German example to found the British Association for the Advancement of Science in 1831[11]. It is no accident that this should be the year before the passing of the first Reform Bill, and that many of the supporters of the British Association should have been the same active radicals who had helped to found Birkbeck College in 1823, and University College, London, in 1827. For the first fifty years of its existence, the Association acted as the driving force of applied science, gave it publicity and even

helped to finance it. Nevertheless, in an age of individualism and before the great monopolies existed to endow foundations, the resources available could go only a very little way to help scientific research or even teaching.

Facilities for learning science were very limited till near the end of the century. No practical science teaching was given in Britain, even in Cambridge, before 1845 and then it grew very slowly. The College of Chemistry—the germ of the later Royal College of Science and of Imperial College—founded in 1845 at the instance of the Prince Consort was an isolated exception. It was found necessary to import a director from Germany, A.W. von Hoffman, one of Liebig's bright young men; and the brilliance of his pupils such as Crookes and Perkin testified to the need for such an institute. Perkin discovered the first aniline dye and laid the foundation of an industry that was soon to be lost to Germany. France had owed her scientific pre-eminence in the early 19th century to the great teaching centres of the Ecole Polytechnique and the Ecole Normale, but even there practical teaching was limited to specially favoured assistants of the professor. Even in Germany practical science teaching only started in a small way in Liebig's laboratory in Giessen after 1825.

In Britain research facilities hardly existed at all. The historian of science is inevitably struck by the unbroken sequence of important discoveries that from 1797 to 1850 and beyond from the Royal Institution, but this was in fact thanks to the foresight of Count Rumford, who established what was practically the only research laboratory in Britain which could call on the very best talent available.

This defect was partly supplied by the existence of a number of private laboratories which wealthy amateurs set up at their own expense. In several of these, such as that of young Joule, a rich brewer's son, or of Strutt, afterwards Lord Rayleigh, a successful manufacturer, work of the greatest importance was carried out. Nevertheless such laboratories could not supply the real need of an advancing science. They were exclusive and limited to the life or even the interest of their owners, and, worst of all, they could not serve as continuous schools of research. Meanwhile professors had to manage as best they could in cellars[12]. The era of big university research laboratories only began in the sixties, inspired largely by Liebig, and spread from Germany very slowly all over the world[13].

The steadily accelerating progress of science and technique in the nineteenth century came about in spite of these disadvantages. The ultimate

driving force was the enormous growth of demand for industrial products as their cost fell. This, in turn, was a consequence of the quantitative spread of the effects of the qualitative change of the industrial revolution in the eighteenth century. The more kinds of goods were made by the new methods, the more openings there were for inventions, and, at one remove, for the science on which they were based. This in turn led to a demand for more science teaching, which provided the necessary sustenance for the growth of academic science.

In other less material ways the intellectual atmosphere of the 19th century favoured the adventure of science. It was the ideology of the rising class of manufacturers, though they were apt to drop it when they had made their pile and moved into high society. It was predominantly liberal, progressive and anti-clerical. Outside Britain and France, where it had been more or less assimilated by the ruling classes, it also had a revolutionary aspect, as in Russia, or a nationalist one as in Poland and Italy.

The nineteenth century witnessed the beginning of a significant spread and shift in the centre of gravity of science. At its outset it was limited to the area of cultivated society of the eighteenth century. This centred on France and Britain, which set the tone, but included small and select groups of liberals in the countries touched by the enlightenment, the Low countries, the German courts, Switzerland and Italy. Subsidiary centres were established in Scandinavia, in Russia and on the eastern seaboard of the United States. In the first decades of the century, owing largely to the after effects of the French Revolution, the scientific prestige of France was at its height. Later, as the reaction to revolutionary thought that had checked but never halted scientific advance in Britain died away, the weight of its commercial supremacy began to tell.

The scientific movement here had something of a radical flavour about it. It was the assertion of the new generation of scientists allied to the industrialists for recognition in the face of the old established oligarchy of the Royal Society. Babbage helped to found the British Association in 1831, as has been told, in order to bring the industrial world and the government into close relation to science. In the first aim he succeeded up to a point, in the second he failed, being a hundred years before his time. Meanwhile French science, though as rich as ever in individual achievement, was marking time and not expanding. The story of Pasteur brings out both its greatness and its limitations.

By the last quarter of the century leadership at least quantitatively in

science had passed to Germany. There, up to 1848, science, despite highly circumscribed official patronage, definitely belonged to the liberal movement of the enlightenment. The somewhat mystical and absurd prophet of Naturphilosophie, Oken, a friend of Goethe, founded, as we have seen, the Society of German Naturalists largely to emphasise the liberal aims of science. In 1819 he resigned his chair rather than censor his scientific political magazine, *Isis*. After 1848, with the compact between the bourgeoisie and the princes, the scientists became pillars of state, scientific education was favoured and magnificent laboratories began to be built. It was from Germany that the next phase of science was to come, with its close links with the new monopoly industries and with the state, particularly in its military aspect.

The development of science in Russia was characteristic of its native genius as well as of its political and economic backwardness. At the beginning of the century science was well-established there in the academies and the universities, owing largely to the initiative and enthusiasm of Lomonosov. All through the century came a succession of brilliant individual scientists, Jacobi, Lobachevski, Lentz, Jablochkov, Mendeleev, Butlerov, Metchnikov, all of whom made important contributions to world science. The advance of science inside the country was, however, doubly hampered by the autocratic government and the essentially feudal social system. Tradition, combined with fear of the revolutionary implications of science, led the government to favour foreign rather than native science, and to fill the academy largely with German scientists. As industry was also largely in the hands of foreign concessionaires, the numerous inventions of Russian scientists were not taken up, so that the potential contribution of Russia to technological advance was not realised during the nineteenth century. Only towards its end the growth of a native capitalism provided conditions for that of science free from foreign domination, but this was only to bear fruit during the next century.

In the United States the conditions were very different. The original scientific impetus of Franklin and Jefferson had largely disappeared by the 19th century. The extreme laisser-faire of Jacksonian democracy did not favour federal or state aid to science, and the colleges and universities were on the whole conservative institutions which looked to Europe for inspiration. The United States did produce some eminent men in the physical sciences in the 19th century, such as Henry and Gibbs, but they had little impact in their own country. The reason was that in a continent being rapidly opened up, with a growing and shifting population, there

was no place for the European type of intellectual nursed in a highly traditional and stratified society.

It was quite otherwise in the technical field. There the conditions were peculiarly favourable for ingenuity. Great resources, shortage of labour, long distances all put premiums on machinery and the greatest degree of automatic working. The inventor who needed neither schooling nor capital had an open field. Agricultural machines, sewing and bootmaking machines, typewriters, revolvers are naturally American labour-saving inventions. Even more significant for the future were the laying of the foundations of fabrication from interchangeable parts, first developed for small arms by Eli Whitney, and that of the assembly line in the slaughter houses of Cincinnati. The coming together of these methods was to engender the mass production of the 20th century. There was nothing like the same need for capital-saving inventions. Here the major advances in steel and chemicals were made in Europe. Even at the end of the century, as the story of Edison shows, there was still far less contact between the professional scientists and the inventor than in Europe[14]. Inventors found plenty of scope in the application of a few simple and old scientific ideas, while scientists remained almost unaffected by the wealth of constructive devices that were coming into use all around them.

There is one aspect of 19th century science that is generally overlooked, because until very recently it was taken completely for granted. It is that science, however much it increased in scope during the century, remained throughout the preserve of a small minority of people in a very small section of the world. Only the industrial countries of Europe and the newly industrialised parts of America contributed to modern science. The rest of Europe, and all of Asia and Africa, were left out, though the exploitation of their peoples was essential to the very existence of industrial capitalism.

Even in the capitalist countries themselves, though the rising manufacturers and engineers won a place for themselves in science, it was still open to a very small section of the population. Samuel Smiles' efforts in *Self Help* to prove the contrary only succeeded in producing pathetic examples of how impossible it was for a working man to be anything but a praiseworthy amateur. Actually as the century wore on it became more and more difficult for the poor outsider to contribute to invention, let alone to science. George Stephenson was the last of the great workmen-inventors in Britain. Edison, for all his casual and self-supporting youth, cannot be called a working man and he was the last of the selftaught

inventors. These national and social limitations must have excluded from contributing to science all but a very small fraction of those competent to do so. In other words, the potential advance of science was many times larger than what was actually achieved.

In the last analysis, social and economic factors, rather than scientific and technical ones, have had the determining effect on the speed of scientific advance. This, however, was only apparent to a very few scientists in the nineteenth century. Indeed the only nineteenth century writers who were able to treat of science in social terms were Marx and Engels. It is only in the light of 20th century experience that the potentialities of ordered scientific advance, using all available forces for socially justifiable ends, are beginning to be realised.

From the vantage point of the conflict and achievement of our time we can see how 19th century science in all its disordered and hampered vitality prepared the way. It was only when that phase had been passed that it was possible to see clearly how science could be used consciously and developed with a purpose. We have seen in our own time how, with the purposes drawn from the old pre-scientific civilisation, and in the interest of a small group of wealthy men, this has resulted in the major perversion of science to destructive ends. But the other potentiality for controlling the world to secure the best conditions for man and at the same time the way to use his capacities to the full, is also becoming more and more evident, and sooner or later social forms will be adjusted to make this an actuality.

## NOTES

1. I had hoped to be able to include illustrations of the detailed connections between science and industry in some particular limited fields, such as those of the conservation of energy, of the development of the electric light, but the result was an essay out of scale with the others in this volume and further enlarged, has become the basis of a separate book: J. D. Bernal, *Science and Industry in the Nineteenth Century*, London, 1953.
2. *Op. cit.*
3. See M. MacLaren, *The Rise of the Electrical Industry during the Nineteenth Century*, Princeton, 1943.
4. Siemens was educated at the Gymnasium at Lübeck, then at the Polytechnic School at Magdeburg, and finally at the University of Göttingen. Gilchrist Thomas studied chemistry at Birkbeck College, London.
5. Sir Charles Parsons once told me that he attributed his success over other turbine designers to the length of the purse of his firm. They spent nearly £ 100,000 on development before getting any returns.

6. Strictly speaking distilling is the first really scientific industry, though its genesis lies outside the scope of this essay. The still was really a laboratory apparatus afterwards blown up to industrial dimensions. The Arabs used it for distilling rose water, the Christians for aqua vitae, usquebaugh, or fire water. The development of this still, which was very rapid from the mid fourteenth century when the drinking of spirits became popular to the mid sixteenth century, coincided with the first rise of experimental chemistry. From then on until the industrial revolution it became stereotyped —a routine operation beneath the notice of the man of science.
7. As Arthur Schuster put it in "Britain's Heritage of Science" (1918) page 180: "In the seventies of last century it was generally thought that our power to discover new experimental facts was practically exhausted. Students were led to believe that the main facts were all known, that the chance of any new discovery being made by experiment was infinitely small, and that, therefore, the work of the experimentalist was confined to devising some means of deciding between rival theories, or by improved methods of measurement finding some small residual effect, which might add a more or less important detail to an accepted theory. Though it was acknowledged that some future Newton might discover some relation between gravitation and electrical or other physical phenomena, there was a general consensus of opinion that none but a mathematician of the highest order could hope to attain any success in that direction. Some open-minded men like Maxwell, Stokes, and Balfour Stewart, would, no doubt, have expressed themselves more cautiously, but there is no doubt that ambitious students all over the world were warned off untrodden fields of research, as if they contained nothing but forbidden, though perhaps, tempting, fruit".
8. See H. G. Wells, *Experiment in Autobiography* and Charles Dickens, *Hard Times*, Ch. 1.
9. See C. Babbage, *Reflections on the Decline of Science, and on some of its causes*, London, 1830.
10. See C. R. Weld, *A History of the Royal Society*, London, 1848, Vol. I, page 146.
11. See O. J. R. Howarth, *The British Association: A Retrospect 1831–1931*, London, 1931.
12. "Thomson sought for the opportunity to follow out experimental research. About the year 1850 an old disused wine-cellar in the College basement ... was taken possession of". From *The Life of Lord Kelvin*, S. P. Thomson.
13. In 1868, Pasteur, who still had no proper laboratory of his own, felt impelled to write an article making a plea for laboratories to save French science: "The boldest conceptions, the most legitimate speculations can be embodied but from the day when they are consecrated by observation and experiment. Laboratories and discoveries are correlative terms; if you suppress laboratories, physical science will become stricken with barrenness and death; it will become mere powerless information instead of a science of progress and futurity; give it back its laboratories, and life, fecundity and power will reappear. Away from their laboratories, physicists and chemists are but disarmed soldiers on a battlefield."
14. There were, of course, notable exceptions. Professor Elihu Thomson's interest in testing electric devices at the Franklin Institute led him to go into business with Edwin J. Houston to form a highly successful company which afterwards joined the Edison General Electric Company to form the present General Electric Company. See M. MacLaren, *op. cit.*

# SCIENCE AND CONFIDENCE IN THE RATIONAL MIND

by

Dorothea Waley Singer*

## I.

### *Renaissance Science: The rise of classical science shatters mediaeval cosmology and philosophy.*

The development of cultural history, whether of art, science, or philosophy, has revealed many examples of the passage of ideas both in space around our whole globe and in time throughout the centuries[1]. Perhaps this is a primary factor in producing the phenomenon, to which Flinders Petrie first drew attention, of the spiral reappearance of certain very definite art-forms in successive cultures, often far removed from one another both in space and time.

A glance at any period of human history and especially at the course of European history of which our knowledge is at present the most intimate will demonstrate a continuous human effort toward the attainment of certainty concerning Man's place in the Universe. That is why changes in cosmological belief have always led to profound disturbance. Moreover, if cosmological and cosmogonical conceptions are to survive, they must be in full harmony with the results attained by the exercise of all human faculties whereby men seek to understand themselves and their place in the Universe.

One of these faculties is the *sense of the numinous*. Another is *reflection*, another is *imagination*. Yet another is *observation*—a faculty that will stimulate the others. Observation of our earth and its non-human inhabitants and of the heavenly bodies is part of what we call science. Very

* Kilmarth, Par, Cornwall.

*Centaurus* 1953: 3: 166–182

soon science extends to observation of Man himself, of his body and its reactions in health and disease, of man's group behaviour and, perhaps last of all, of the inter-relationship of the rational and irrational factors that determine both individual and group behaviour.

The less the information that has been gathered by observation, the more its place is taken by the other human faculties listed above. In the ancient world, much reflection and argument were built upon insufficient or erroneous observation. In the Scholastic period, interest was concentrated on the process of argument. The thoughts of man, as revealing the thoughts of God, were felt to offer a more compelling subject of study than any physical objects.

What was the change in the European mind that led to the outburst of constructive energy that we call the Renaissance? As regards science, what was the basis of the renewed interest in the physical world and of the desire to understand more and more of natural processes? How far did science promote, and how far was it the result of, this upsurge of joyful confidence? The present generation can verify from experience that periods of sudden change of environment are potent factors in stimulating a psychology that expects and welcomes further change. Moreover, discovery begets the spirit of exploration, both in the physical and the metaphysical field. The gradual supercession of the feudal system by a novel political framework to society, the rise of the Reformed religions, the development of the nation-states, the tales, no less than the treasures, brought home from distant voyages, all these provoked "new thoughts" and quickened interest in the physical world. The process was reinforced by its own success. The growth of scientific knowledge, partly through the rediscovery of lost books in which such knowledge was recorded, but yet more by persistent, planned and indefatigable research, through the growth of technological skills[2], with the increasing perfection of such instruments as the telescope and the microscope and the wide dispersal of the printed page—all opened the way for further scientific study and achievement.

Undoubtedly, the growth both of scientific knowledge and of scientific achievement were factors in the joyous selfconfidence that characterised the Renaissance period, stimulating yet further activity. Reflection became diverted to the integration of the new observations into the world view, in spite of the tendency of the Platonists toward an exaggerated dichotomy between the material and the spiritual[3]. Here again we may discern the special contribution of science, which called no less for accurate re-

cord of material observations (sense-data) than for the use of the mind in their interpretation and in devising further experiments or fields of observation.

No longer did Man feel himself beset by the horrid and allpowerful Erinyes or other personifications of Fate. Divine foreknowledge was conceived as embracing the fate that man brings on himself, not the destiny planned for him on high. The middle ages had regarded Man the Microcosm as a reflection or shadow of the universe, the Macrocosm. Astrology furnished one key to this intimate linkage of microcosm and macrocosm, and was therefore regarded as an eminently rational body of knowledge, based on observation and experience. In fact, it may be said to have been an imperfect but genuine expression of the scientific approach to cosmology. To the Church, astrology had been apt to appear as an insolent attempt to substitute natural law (to use our modern term) for divine omnipotence, though the Church ultimately came to terms with astrology. Very gradually, with the accumulation of further observation and experience, astrology came to be recognised as a vast delusion.

But there remained the problem of free-will. How could man's evident power to mould his own fate be reconciled with the divine power to alter the course of nature and the sequence of cause and effect, a power of which the exercise was recorded in Holy Writ? Renaissance man's solution of this problem was very different from that of the Book of Job. Man had bred the war-horse. He had, by the elimination of the infestation of sheep, made it possible for the flocks to multiply. By proclaiming, and still more by fulfilling, the duty of disinterested search for objective truth, scientists contributed to respect not only for mind but also for intellectual integrity, and hence for justice, which is meaningless unless reposing on intellectual integrity. Let us hear the testimony of some of the Renaissance scientists. The choice of names must be arbitrary, and we can select only a few of those who most typically illustrate our theme.

We turn to Charles de Bovilles (1470–1535), a writer of diversified talent, author of the first Geometry written in the French language. His work *On Wisdom* presents an extraordinary combination of mediaeval thought with insurgent humanism[4]. The discussion of macrocosm and microcosm and of the functions of the angelic hosts are in full mediaeval style. Elaborate figures and tables of qualities are reminiscent of Lull, while the use of symbolism based on the Trinity recalls Cusanus[5]. But the grand theme of the work is, that Man has been endowed by God with Mind whereby he may through Wisdom attain to unity with the very God-

head. The thesis is propounded at once in the dedication to Guillaume Briçonnet (Bishop of Lodève and subsequently of Meaux), which is illustrated by a most interesting woodcut replete with messages signifying that man must by Knowledge be master of his fate. In the foreground are the seated figures of *Fortuna* and *Sapientia*. *Fortuna* is blindfolded and bears a revolving wheel in her hand. Her seat is an unstable sphere and above her the head of *Insipiens* announces: "Thee, o Fortune, do we make our goddess, and place thee in Heaven". Wisdom, on the other hand, is seated on a four-square throne and gazes serenely into the mirror of Wisdom which she bears in her hand. Above her is a head labelled *Sapiens* who proclaims: "Put thy faith in thine innate strength (*virtuti*): fortune is more fleeting than waters (*undis*)".

We will next consider two writers who, while not themselves practitioners of science, were yet profoundly influenced by science. Each of them evolved a philosophy of science that exercised great influence on both contemporary and succeeding generations. The first of these was Giordano Bruno (1548–1600). He has a particular bearing on our enquiry, for while a general faith in human reason (in so far as it did not prevent acceptance of the current theology) was axiomatic to his predecessors and his contemporaries, Bruno was a pioneer in the recognition that a true interpretation of sense-data is often only attainable by a profound effort of Reason. In his greatest latin work[6], he describes an episode of his childhood which had made a deep impression on him and had led him to understand this relationship between sense-impressions and Reason. Bruno's home was in a hamlet just outside Nola, on the lower slopes of Cicada, a foothill of the Appenines, some twenty miles east of Naples. He tells us with affectionate detail of the beauty of the land around, overlooked from afar by the seemingly stern, bare steeps of Vesuvius. One day a suspicion of the deceptiveness of appearances dawned on the boy. Mount Cicada, he tells us, assured him that "brother Vesuvius" was no less beautiful and fertile than itself. So girding his loins he climbed the opposite mountain. "Look now" said Brother Vesuvius, "look at Brother Cicada, dark and drear against the sky". The boy assured Vesuvius that such also was his appearance, viewed from Cicada. "Thus did his parents [the two mountains] first teach the lad to doubt, and revealed to him how distance changes the face of things". So in after-life he interprets the experience, and continues: "In whatever region of the globe I may be, I shall realise that both Time and Place are similarly distant from me". Reflection on this childhood experience prepared Bruno for

his gradual formulation of the philosophy which was the leading motive of his stormy life-course and of the ethic that he built on this philosophy.

His whole view was conditioned by the conception of an *infinite universe*. This he received from the works of two writers who occupy opposite philosophical poles—Lucretius (B.C. 96–55) and Nicolaus of Cusa (1401–1464)—Lucretius denying the validity of theological or metaphysical thinking, while Nicolaus sought in his cosmology and even in his physical experiments[7] a reinforcement of his theology.

Bruno, however, could base his conception of an infinite universe also on the observations of contemporary astronomers, such as Tycho who had in 1572 observed the motion of a new star and in 1577 a great comet, both in the "ethereal regions"—which thus lost the immutable character ascribed to them in the older view.

Moreover, Bruno declares: "These things were discovered by me some lustres back and were proved by reason ['interior sense']. But now at last I may accept that they are confirmed by the learned Dane Tycho who by his wise talent hath discovered many things"[8].

Bruno needed no stimulus to daring and uninhibited expression of his thought, though doubtless he rejoiced in the Lucretian assertion of man's freedom and indeed obligation to follow unflinchingly the implications of his vision of a single, infinite universe. From Lucretius and certain Renaissance Lucretians such as Fracastoro (1483–1553), he drew his conception of what he calls the *Minima* from which all things are formed. The diverse multiplicity of phenomena he attributed to the grouping of these minima, which are in eternal motion, constantly leaving yet constantly tending to return to "their own natural body and place". Thus he envisaged an eternal process of what we may call *cosmic metabolism*[9].

"From the Minimum everything groweth and every magnitude is reduced to the minimum"; and "the minimum buildeth up to the many and to the innumerable and infinite"[10].

"As semina are aggregated round bodies, atoms are added to adjacent parts, so the body with its members taketh its rise; but as these parts are expelled from the centre, so the bodies, however well knit, are gradually dissolved"[11].

"When we consider ... the being and substance of that universe in which we are immutably set, we shall discover that neither we ourselves nor any substance doth suffer death; for nothing is in fact diminished in its substance, but all things, wandering through infinite space, undergo change of aspect"[12].

It was doubtless from the writings of Nicolaus of Cusa that Bruno derived the conception of the *Convergence of Contraries* within the universal infinite unity[13]. With Nicolaus, he held that an infinite universe can provide no absolute position, no centre or circumference, but that the position of our world or of any object within or without it can be defined only in relation to another object. Moreover, the subject-object relationship was envisaged by Bruno as a process of admixture culminating in identity.

"Our philosophy ... reduceth to a single origin and refereth to a single end, and maketh contraries to coincide so that there is one primal foundation both of origin and of end"[14].

"There are many dissimilar finite bodies within a single infinity ... many continuous parts form a unity ... as with liquid mud. There throughout and in every part, water is continuous with water, earthy matter with earthy matter; wherefore, since the concourse of the atoms of earth, and the atoms of water, is beyond our sensible apprehension, these *minima* are then regarded as neither discrete nor continuous; but as forming a single continuum which is neither water nor earth ..."[15].

Bruno's conception of matter is, again like Cusanus', illuminated by analogy both from geometry and from number. Following the fantasy of Raymond Lull, Bruno uses as symbols of thought complex geometric figures. Congenial too to Bruno are the analogies drawn by Nicolaus of Cusa from the growth of infinite mathematical series, arising from unity. Moreover Bruno finds in mathematical theory support for his conception of the indivisible atom or monad[16]. From such thoughts, Bruno passed to his remarkable synthesis of universal relativity of our sense-data both of Time and Space within the unity of the infinite universe.

In the universe so conceived, there follows naturally Bruno's conception that we have called *Inherent Necessity*. All motion and indeed all changes of state he ascribes to the inevitable reaction of a given body to its environment, but he does not conceive merely an external environment acting on an inner nature but rather regards the force leading to change in a given body as a function of the body itself, whose fundamental nature (*raggione*) includes reaction in a particular manner to a particular environment, thereby exercising its influence on that environment and thus ultimately on the whole universe. Thus the freely developing *raggione* or inherent nature of every part of the universe is influenced by and exercises its influence on the *raggione* of every other part. But it is this *raggione* or ultimate nature, rather than the detailed behaviour of each part, that suffers and exerts influence.

Bruno's vision of all things impelled to action according to their essential nature fitted his assertion of Man's inborn right to follow the dictates of his own soul.

"The divine nature of the soul is perceived, nor doth any passion or change take place therein. To whatever fate she is subject coming to the part of a composite whole, she hardly remaineth for one moment affected by the same fate, yet she remaineth steadfast as a single entity ... for the judgements of inexorable fortune dwell in the soul; never did any grief or any joy suffice to tear man from his station"[17].

Thus the Lucretian universe of innumerable minimal parts or atoms in perpetual concourse and discourse, became for Bruno the symbol of the spiritual universe of an infinity of monads. All partake in the World Soul which, too, is an infinite continuum; yet in another sense discontinuous and divisible and even (on the analogy of number though not with unvarying consistency) infinitely divisible.

This conception again was symbolic of his view of the human soul, every individual soaring to the uttermost heights of thought and spiritual development congruous with his own nature, every individual imbued with the divine spirit, whereby the infinity of discrete and independent souls is yet fused into a vast whole transcending their discrete separateness, a unity informing infinite space and eternal time, the World Soul, governed by Mind or, as he sometimes says, Wisdom.

Exaltation of Mind is the major theme that emerges (after many diversions) in the ethical works in the Italian tongue that followed Bruno's three slim volumes—also in the vernacular—on cosmology and philosophy during his happy few years in London. In the heaven reorganised by Jove, we learn, "Supreme Truth ... occupies the most exalted position in Heaven"[18]. But cosmology, philosophy and ethics are intermingled in all Bruno's works as in his thought. "Mind moveth the whole form and is poured into the limbs and mixeth itself throughout the body"[19]. Bruno reiterates his conviction that the immediate interpretation of our sense-impressions may lead us far astray, while on the other hand our imagination, though it may set us on a right track, may similarly be completely deceptive. Only by enthroning reason as arbiter can we reconcile imaginative experience with sense-perception:

"No corporeal sense can perceive the infinite. None of our senses could be expected to furnish this conclusion; for the infinite cannot be the object of sense-perception; therefore he who demandeth to obtain this knowledge through sense is as one who would desire to see with his eyes both

substance and essence. And he who would deny the existence of a thing merely because it cannot be apprehended by the senses nor is visible, would presently be led to the denial of his own substance and being. There must be then some measure in the demand for evidence from sense-perception, for this we can accept only with regard to sensible objects; and even then it is not above all suspicion unless it cometh before the court aided by good judgement. It is for the intellect to judge, yielding due weight to factors absent and separated by distance of time and space. And in this matter our sense-perception sufficeth us and yieldeth us adequate testimony, since it is unable to gainsay us. Moreover, sense advertiseth and confesseth his own feebleness and inadequacy by the impression it giveth us of a finite horizon, an impression which is ever changing. Since then we have experience that sense-perception deceiveth us concerning the surface of this globe on which we live, much more should we hold suspect the impression it giveth us of a limit to the starry sphere.

"Of what use then are the senses to us?"

"Solely to stimulate our reason, to accuse, to indicate, to testify in part; not to testify completely, still less to judge or condemn. For our sense-perceptions, however perfect, are never altogether undisturbed. Wherefore truth is in but very small degree derived from the senses, as from a frail origin, and doth by no means reside in the senses.

"Where then resideth truth?"

"In the sensible object as in a mirror. In reason, by process of argument and discussion; in the intellect, either through origin or by conclusion; in the mind, in its proper and vital form"[20].

Very different from the wandering refugee genius Giordano Bruno, was Lord Chancellor Francis Bacon, Lord Verulam (1561–1626)[21] who claimed that by proper organisation science both could and should be applied by man to transform the material conditions of life.

"It is well to observe" he writes, "the force and effect and consequences of discoveries. These are to be seen nowhere more conspicuously than in those three which were unknown to the ancients, and of which the origin, though recent, is obscure; namely, printing, gunpowder, and the magnet. For these three have changed the whole face and state of things throughout the world; the first in literature, the second in warfare, the third in navigation; whence have followed innumerable changes; insomuch that no empire, no sect, no star seems to have exerted greater power and influence in human affairs than these changes"[22].

Again, Bacon writes of his project:

"What is at stake is not merely a mental satisfaction but the very reality of man's wellbeing, and all his power of action. Man is the helper and interpreter of Nature. He can only act and understand in so far as by working upon her and observing her he has come to perceive her order. Beyond this he has neither knowledge nor power. For there is no strength that can break the causal chain: Nature cannot be conquered but by obeying her. Accordingly these twin goals, human science and human power, come in the end to one. To be ignorant of causes is to be frustrated in action"[23].

In spite of the events which clouded the close of Bacon's life, his writings exercised a profound influence, and led to the foundation of the Royal Society. The first account of the Royal Society bears a portrait of Bacon, and his name was cited constantly at the meetings throughout the 17th and 18th centuries.

The importance of his contribution to the philosophy of science is destroyed neither by the absence of a significant addition by Bacon to our knowledge of the natural world, nor by his failure to propound a successful blueprint for the organisation of which he so clearly saw that human welfare stood in need. His influence is active to-day[24] and may be traced in the works of the most diverse among contemporary writers who are both cognisant of scientific achievement and concerned with human welfare, the right ordering of human life, and precisely the subject considered in this volume, science in relation to the general life of man. It has of course not always been those with such a preoccupation whose work has in fact been most crucial to the scientific contribution, for it is a commonplace of scientific history that developments of our knowledge of fundamental science have with remarkable consistency led to applications yielding the most dramatic easement to the conditions of human life. Such physical easements are not the only gifts of natural knowledge to the non-scientific man. We do not need citations to remind us that the confident sense of an ordered universe partly destroyed by Renaissance astronomy was rebuilt partly by the very men whose work had been catastrophic to the older views. Galileo and his successors established laws of physics which were steadily reinforced until with Newton the awe-inspiring view was reached of a single natural physical law pervading the whole universe.

Meanwhile biologists were building up a no less all-embracing biological scheme which reached its apotheosis in the work of Charles Darwin.

Both physical and biological concepts were to suffer startling modification. As in the opening constructive years, so with the passing of the era of classical science, the scientific disciplines, in reshaping their own conceptions, profoundly disturbed the general course of philosophic thought.

## II.
## *The Twentieth Century: Transformation of Classical Science transforms current Philosophy.*

We have glanced at scientific discovery as both cause and result of the great upsurge of confidence in the human mind and consequent release of joyous energy at the opening of the period of classical science. The course of science from the 16th to the early 20th century and its influence on the development of philosophical thought during those centuries has occupied and is occupying many skilful pens and many learned minds. We turn to the close of the period, for it may perhaps be of interest to historians of science to receive an impression from one of the diminishing number of eye-witnesses of the impact on educated but non-scientific minds at the close of the age of classical science of the new physics, heralded by the announcement of the experimental confirmation of Einstein's relativity theory. Few outside the small circle of researchers in physics had previously any conception of the current development of physical theory.

The late 19th and early 20th centuries had witnessed dramatic achievements from the application of scientific discovery. We need but recall the application of electricity, first to provide smokeless illumination, then effortless fuel; the internal combustion engine that revolutionised transport, the telephone that had brought communication between opposite hemispheres, factory machinery that was believed to be heralding a world of abundance in which all could share; the conquest of successive diseases by inoculation, X-rays and other newly devised medical weapons, and the achievements of public hygiene that offered hope of yet further alleviation of man's lot.

It is within the experience of each one of us that every new skill, every new power, brings exhilaration and increased self-confidence. But is this self-confidence confidence in human reason? On the contrary, is not increase of power (whether or not coming as a *result* of scientific knowledge) a temptation to irrational action, which becomes possible precisely

through accretion of power? Now in the 19th century there arose, more especially in England and more especially for those who commanded money, enormously increased power to enjoy a far more diversified life than before. Not only did science supply the diversity, but scientific knowledge (applied, for example, in factories) was recognised as having made possible the acquisition of the riches that were indispensable for the enjoyment of the new facilities. And at that period, it was taken as axiomatic that science, like every other branch of knowledge, was gained by the exercise of human reason to interpret observation and experience. We see a reflection of this view in all the science myths of the classical period—Galileo and the Tower of Pisa, Newton and the falling apple, Stephenson and the kettle. In fact, it was this combination of alert observation with reflection leading to rational interpretation that was held to constitute the achievement of the passage from scholastic ratiocination to the modern command over nature.

In those days the Unconscious and the Irrational were generally believed to motivate only the unlettered and the unreflective—in fact, those persons who were excluded from participation in the gifts of science. Not that this exclusion was accepted with equanimity either by the excluded or by all of the fortunate. On the contrary, to consider only England, this was the era of University Settlements designed precisely to give the pleasures of culture to those described to-day as "educationally under-privileged"; of polytechnics, designed both to widen the circle of enjoyment of culture and also to provide an economically helpful "ladder of learning"; of university extension lectures—fore-runners of the W.E.A.; of the foundation of the Working Men's College, the People's Palace in London, and a large number of what we may generically call Mutual Improvement Societies, founded by and for working men for the purpose of study.

Nevertheless, there was already the beginning of an anti-intellectual tendency. In England it was first observed by the present writer as a young arrival to a university circle as long ago as in 1914 and the succeeding war years. It was no less surprising than startling to notice that the bearers of revered names associated with intellectual achievement were mainly and almost youthfully happy, not because this achievement proved of importance in the national emergency, but because they themselves, quite apart from such achievement, found that they were, in the current phrase, "of practical use in the war".

Some personal gratification under such circumstances was perhaps

inevitable. But we are here considering rather the public aspect which might be described as gratification that events appeared to justify a passionate belief in general culture as of an importance completely overriding that of specialised knowledge such as was acquired, by the scientist. This belief was at the time generally accepted among persons of liberal education: it was manifested, for example, in the status of men of general culture in the governmental hierarchy. It was in fact an expression of passionate faith in Mind. Perhaps that adjective *passionate* may suggest to us some underlying causes of the subtle change that presently was observable among the circles we are considering. Psychology has demonstrated that passion not uncommonly replaces certainty in the formulation of opinion. Now the more successful and at home the intellectual becomes in dealing with practical affairs, the more highly he is apt to evaluate "practical achievement" as against so-called "theory", or—to use the modern phrase current among scientists—as against those additions to fundamental knowledge on which the practical achievement must ultimately be based. This fundamental knowledge can normally be won only by strenuous and exhausting effort, by the exercise of certain innate intellectual faculties and acquired habits of thought which do not become more easily evoked and exercised by the individual who has for an appreciable period laid them aside in favour of so-called "practical" applications of knowledge[25].

Thus, by a curious inversion, it happened to some among the very persons who had hailed their own success as a vindication of Mind, that they then found that they could fulfill their war functions satisfactorily with less mental effort than had been needed for the work of their youth, while the passage of the years did not find them easily able to return to their former creative mental effort.

But the origin of the process that thus unfolded to a young and superficial observer in the years 1914–18 can of course be traced to a much earlier date and to more complex causes. Among these, probably none was more effective than the developments of misunderstood Pragmatism. Interesting light is thrown on this by a work published in London in 1912 under the pseudonym of Vernon Lee with the title *Vital Lies: Studies of some varieties of recent obscurantism.* With cogent argument, though with an unfortunately repetitious style, Vernon Lee inveighs against what she calls the "Will-to-believe" element of Pragmatism. Her work was welcomed both in England and France, though she rightly saw that the doctrine of James took a more fascinating and no less dangerous development in the

work of Bergson. Indeed at the close of the 1914–18 war an Anglo-French Philosophical Congress was held at Oxford at which Bergson used his rapier-like intellect to deliver an exquisitely graceful and completely misleading attack both on intellect and on science. It is difficult to credit that he really believed in the caricature that he presented on this occasion, of scientific discovery as a process of blind logic. To those familiar with the scientific history of the 16th and 17th centuries, his address was strangely reminiscent of the theological reaction to the cosmological heresies introduced by the insurgent science of the earlier period. But to theological Authority in those earlier centuries the new physical theory was familiar and profoundly interesting. Could the works of Bruno and of Galileo have been as limited in their circulation as were the writings of Nicolaus of Cusa, it is probable that their authors would have been as free from exalted censure as was the great 15th century Cardinal. But the British and French philosophers (as distinguished from certain medical psychologists also present at that Oxford Congress in 1919) were frankly and completely bewildered by the philosophic implications which they believed to be implicit in relativity physics.

This leads us to another strange situation in the ancient English universities at that time. To-day, when the "humanities" and science exercise such a potent and fertilising influence on one another, it is difficult to realise the mutual ignorance and contempt that was common between the devotees of these two branches of study, a short thirty-five years ago. The 1914–18 war had partially changed this state of affairs. My nation had indeed learnt of the terrific danger it had narrowly escaped owing to a Prime Minister's refusal to credit the scientists' strange tale of the connection between cotton-wool and explosives. The Oxford tradition of disinterested search for truth now asserted itself. To many leading philosophers, experience during the war years had brought a new insight into the place of science in the Organon of knowledge. It was perhaps somewhat unfortunate that the scientists were now welcomed not only as purveyors of natural knowledge but also as exponents of an interpretation of this knowledge, for which they had not undergone the discipline of philosophic study. Two astronomers especially, Eddington and Jeans, were almost prepared to assure the lay public of the relativity of all human knowledge. For other writers, this soon carried the implication of the evanescence of all human values. It should never be forgotten that these philosophic deductions from relativity theory were always resolutely disclaimed by its author, Albert Einstein.

But by the less educated lay public they were welcomed, and they afforded a rationalisation of the relaxed standards of integrity that are insidious in conditions of warfare. It is well known that misunderstood learning leads to the growth of myth. In recent years, we have had a unique opportunity to witness the process as applied to relativity physics and emphasised by the Heisenberg principle of uncertainty. Perhaps it is no accident that, in a recent study of Myth, the process by which experience gives rise to myth is contrasted with the scientific interpretation of experience. "If science ... reduces the chaos of perceptions to an order in which typical events take place according to universal laws, the instrument of this conversion from chaos to order is the postulate of causality"[26]. Science repudiated the validity of the myth as an explanation of the Universe. But myth making was an expression of emotional experience. Moreover, the science-myth was emotionally satisfactory to some scientists while others, who clearly saw that myth-making and scientific method are incompatible, recognised the scientist's need of an expression for his emotional experience that would not directly violate the laws of his scientific thought.

Thereupon symbolism took charge[27] and was acclaimed, but by its very suppleness symbolism presents danger to intellectual integrity and thereby threatens the scientific mode. Perhaps nevertheless we must turn to art to find an expression of imagination fully consistent with the principle of causality. May a layman, not qualified to give an opinion, yet express a hope that perhaps this has been or is being found along the path of Niels Bohr's *Principle of Complementariness* to the "Unity which enchants" of Bruno's steadfast vision?

It has been the privilege of cultural history to play a major part in breaking down the mutual ignorance and contempt that has tended to impede mankind from the enjoyment of this comprehensive vision. Especially it was the deliberate aim of the pioneers of the history of Science to open the eyes of the scholar to the different but rewarding fruit of the scientist's search for knowledge (in those days it was permitted to call it, search for truth), while furnishing to the scientist a reminder that science provides only one, albeit a precious, avenue in the same search.

In conclusion, let us note the wise words of a distinguished contemporary physicist and scholar:

"Des faits tels que le renouvellement complet des sciences voisines, comme l'astronomie, la chimie et déjà la biologie, font éclater aux yeux

que la physique nous apporte aujourd'hui une synthèse incomparablement plus riche, plus solide et mieux ordonnée qu'il y a cinquante ans. La science n'est pas achevée, bien sûr. Notre connaissance des choses n'est ni complète, ni parfaitement claire, et ne le sera probablement jamais. Mais l'existence même de difficultés définies, de problèmes bien posés qui recevront un jour une solution, bref toute la structure des sciences de la nature démontre de manière éclatante ce qui pour nous est peut-être l'essentiel: que Vérité n'est pas morte"[28].

## NOTES

1. As an illustration of this inter-relationship, we may recall a recipe for use with sore eyes, found in a late Middle English housewife's commonplace book: this recipe, lacking only the little flower common in England where it is known as Eye-bright, is found in many medieval medical works, in the Salernitan poems, and may be traced to a Greek text which was translated into Anglo-Saxon under the Greek title *Peri Didaxeon*.
2. No attempt is made here to take sides in the oft-raised but surely barren controversy as to whether intellectual curiosity or practical need has been the primary stimulus to scientific study. The writer is in fact convinced that there is no universal sequence in the matter, and that both sequence and emphasis of the two motives have been different at different times and places.
3. L. L. Whyte surprisingly regards this dichotomy as a specifically Renaissance development. Cf. *The Next Development of Man*, London, 1944.
4. The *De Sapiente* is printed in *Caroli Bovilli Opera*, Paris and Amiens, 1510–11. It is reproduced from this edition, with introduction and notes by R. Klibansky, as an Appendix to E. Cassirer, *Individuum und Kosmos in der Philosophie der Renaissance*, Leipzig, 1927, pp. 299–412, 453–8, in *Studien der Bibliothek Warburg*, edited by Fritz Saxl.
5. See below, note 13. De Bovilles cites also Cusanus.
6. *De innumerabilibus, immenso et infigurabili; seu de universo et Mundis, libri octo*, Lib. III, Cap. 1. This work was issued together with Bruno's *De Monade, numero et figura, liber consequens quinque de minimo, magno et mensura*, by his faithful Frankfurt publishers John Wechel and Peter Fischer who enabled him to see it through the press himself in 1591, before the doors of the Inquisition closed on him. The volume was re-issued by Jacob Fischer in Frankfurt, 1614. The *De immenso et innumerabilibus seu de universo et mundis* was edited by Fiorentino in Vol. I, Parts i and ii, of the National Edition, *Jordani Bruni Nolani, Opera Latine conscripta*, three volumes, Naples and Florence, 1879–1891, edited by Fiorentino, Tocco and Vitelli, Imbriano and Tallarigo. For the passage here cited, cf. *Op. lat.*, I, i, 313.
7. Cf. especially *De docta ignorantia*, Lib. II (first published in *Opuscula varia, s. l. et a.*, perhaps *circa* 1489. Cf. also *Idiotae*, Lib. III, *De Mente; De Coniecturis*, Lib. I Cap. 11, with figure; *De Venatione Sapientiae*, etc. The most accessible edition of the works of Cusanus is the *Opera Omnia*, 2 vols., Bâle, 1565.

8. *De immenso*, Lib. I, Cap. 5 (*Op. lat.*, I, i, 218–9). Tycho Brahe (1546–1601), Danish astronomer, opened his career by observing a new star in Cassiopoeia on November 11th, 1572, of which he printed an account in the following year. From 1576 he systematically studied the heavens for 21 years at his famous laboratory *Urania* on the Baltic Island of Hveen. In 1588 Tycho published his own system of the world. The earth is the centre of it and centre also of the orbits of sun, moon and fixed stars. The sun is the centre of the orbits of the 5 planets. This system is a mere alternative to that of Copernicus, since all the computations of the positions of heavenly bodies are identical for the two. In Tycho's diagram of the universe, the stars are represented in a sphere. His universe was thus Ptolemaic *and* Copernican.

For other astronomers cited by Bruno, and the English astronomers whom he met, cf. D. Waley Singer, *Giordano Bruno of Nola ...*, chapter 3 [for full title, see next note].

9. Cf. D. Waley Singer, *The Cosmology of Giordano Bruno*, Isis 23, Part 2, No. 88, June 1941, pp. 187–96; and *Giordano Bruno of Nola (1548–1600): his life and thought, with annotated translation of his work "On the Infinite Universe and Worlds"*, New York (Schuman); and London (Constable), 1950.
10. *De minimo*, Lib. III, Cap. 2, Tit., and Lib. IV, Cap. 1, Tit., (*Op. lat.* I, iii, 237 and 269).
11. *De immenso*, Lib. II, Cap. 5, (*Op. lat.* I, i, 273).
12. *De l'infinito universo et mondi*, "Introductory epistle", (Argument of the fifth Dialogue), p. 24 (Gentile, *Op. ital.* I, 303).
13. Nicolaus derived this conception from the 5th century church Father known as Pseudo-Dionysius the Areopagite, who is cited also both by de Bovilles (see page 168) and by Bruno. From Bruno the doctrine passed to his acrid critic Hegel, and hence to contemporary dialectic materialism; a descent that seems itself to furnish an illustration of the doctrine that would surely have appeared no less strange to the 5th century source than perhaps it may to some contemporary disciples of the doctrine.
14. *De l'infinito universo et mondi*, Dial. V, p. 163. (Gentile, *Op. ital.* I, 409–10; Lagarde, *Op. ital.*, 393).
15. *De l'infinite universo et mondi*, Dial. II, pp. 51–52 (Gentile, *Op. ital.*, I, 323–4; Lagarde, I, 334).
16. Cf. especially *De minimo* (*Op. lat. Giordani Bruni Nolani*, I, iii, 119–361; and *De monade, numero et figura* (*Op. lat.*, I, ii, 319–473, 484).
17. *De minimo*, Lib. II, Cap. 6 (*Op. lat.* I, iii, 208–9).
18. *Spaccio de la Bestia trionfante*, *Epistola dedicatoria* [to Sir Philip Sidney] p. 19; Gentile, *Op. ital.* II, 14; Lagarde, II, 412.
19. *De la causa, principio et uno*, Dial. II, p. 39 (Gentile, *Op. ital.* I, 179; Lagarde, *Op. ital.* I, 231).
20. *De l'infinito universo et mondi*, Dial. I, pp. 2–3. Cf. also the magnificent poem to *Mens* at the opening of the *De Immenso et innumerabilibus*, (*Op. lat.*, I, i, 201–2).
21. Bacon mentions Bruno only slightingly [*Novum Organum* (1620) I, Aphorism 45]; but phrases have been noted in his *Novum Organum*, Lib. I, Ch. 84 (and also writings of Galileo and some others) that are almost transcripts of Bruno's writings. Cf. G. Gentilis *Veritas filia temporis*, *Postilla Bruniana* in *Scritti varii di erudizione e di critica in onore di R. Renier*, Turin, 1912.
22. *Novum Organum*, Aphorism 129.
23. Francis, Lord Verulam, *The Great Instauration*, London, 1620.

24. The stimulus of Bacon on his latest biographer is patent and infectious: see B. Farrington, *Francis Bacon, Philosopher of Industrial Science*, New York, 1950, London 1951.
25. Incidentally, a very little reflexion will convince us that if theory appears inconsistent with practice, either the theory is wrongly conceived or the practice is wrongly observed; for true and correct theory cannot be at variance with practice properly understood and properly reported.
26. H. and A. Frankfort, *Before Philosophy*, Pelican Books, London, 1949, pp. 23–4.
27. Cf. Martin Johnson, *Art and Scientific Thought*, London, 1944. Very interesting and relevant exposition concerning the function of Symbolism is in the same author's Eddington lecture *Time and Universe for the Scientific Conscience*, Cambridge, 1952.
28. Edmond Bauer, *L'Electromagnétisme hier et aujourd'hui*, Paris, 1949, pp. 332–3.